AN EMPIRICAL STUDY OF CERTAIN TESTS FOR INDIVIDUAL DIFFERENCES

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PREFACE

In the spring of 1911 the writer was called on by the Coca-Cola Company, of Atlanta, Ga., for an opinion as to the influence of caffein on mental and motor processes. In the absence of adequate reliable data (see discussion of previous investigations) it seemed necessary to conduct a set of careful experiment before any opinion could be rendered with either fairness r inty. Such an investigation was made possible by an approp ion by the Coca-Cola Company sufficient to cover all the expenses of the experiments. A later appropriation made possible the publication of this monograph, which presents in full the results of that investigation, a preliminary oral report of which was made by the writer in the U. S. Court at Chattanooga in March, 1911.

The writer is well aware of a popular tendency to discredit the results of investigations financed by commercial firms, especially if such concerns are likely to be either directly or indirectly interested in the outcome of the experiments. He is also aware of a similar human impulse at once to attribute interpretative bias to the investigator whose labors are supported and made possible by the financial aid of a business corporation, and hence do not represent a vicarious sacrifice of time and effort on his own part.

From the point of view of the immediate data any such bias can easily be avoided by having the measurements made and recorded by assistants who know neither the experimental conditions under which the records are being made nor the direction in which the facts may be pointing. If these data are then presented in full they may receive independent interpretation by any one who is inclined to take the pains to examine them. Such conditions were adhered to throughout the experiments to be reported here, and the immediate data are given in full. Thus in no case did any assistant know whether the measurement being made was a caffein record or a control record (see chapter on method), and separate tables are given which present all these records.

But the monograph would be relatively useless were no attempt made to interpret the data. The writer has therefore given the conclusions based on his own careful study of the
clusions are, to the best of his ability, free
bias. While he was c

the experiments themselves and to the preliminary oral report, the considerable labor involved in preparing the results for publication is entirely his own contribution, and was undertaken on his own initiative. The invitation to direct such an investigation provided opportunity for a most valuable addition to scientific knowledge of the effects of the substance specifically studied; for a careful examination into the value of various sorts of tests for the purposes of such study; and for the accumulation of a great mass of data on a variety of problems of intense psychological interest. To have refused this opportunity to make a useful contribution to knowledge, and to hesitate to interpret the results of the study, simply through fear of the suspicion of bias, would have been nothing less than an evasion of scientific duty.

In the light of these statements the reader must place his own estimate on the ability of the writer to free his interpretation of all suggestion of bias. The complete data are given. They have been compared from several points of view and by various methods of computation. The conditions of each experiment are explicitly stated. Conclusions can thus be checked up without difficulty by reference to the records themselves, or somewhat more inconveniently by a repetition of the experiments reported.

H. L. Hollingworth.

Columbia University.

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APPENDIX

AN EMPIRICAL STUDY OF CERTAIN TESTS FOR INDIVIDUAL DIFFERENCES

I

HISTORY OF THE INTEREST IN INDIVIDUAL DIFFERENCES

1. The Work of Various Investigators

The history of scientific inquiry into the nature and amount of individual differences dates back only about twenty-five years. Before that time experimental psychology had concerned itself chiefly with investigations into typical mental functions, especially those of perceiving the external world. For this purpose long and detailed tests were made upon a very few, or perhaps a single subject.

Galton in England was the first who devised and applied a series of tests, both physical and mental, to large numbers of subjects with a view to determining norms and studying the amount, causes and kinds of variation. Since the publication in 1883 of Galton's "Inquiries into the Human Faculty and its Development," the work done in this field in England has been chiefly confined in its application to school children; witness Bryant's experiments in 1886 in testing the character of school children[1] and the more recent work of Winch,[2] Spearman,[3] W. G. Smith,[4] Wimms,[5] and Burt.[6]

In Germany there is the general work of Münsterberg in 1891,[7] Kraepelin,[8] Aschaffenberg,[9] and Oehrn in 1896,[10] Cron in 1897,[11]

[1] *Journal of the Anthr. Inst. of Gr. Britain and Ireland.*

[2] *Brit. Jour. of Psych.*, **1**, 1904.

[3] *Am. Jour. of Psych.*, **15**, 1904.

[4] *Brit. Jour. of Psych.*, **1**, 1905.

[5] *Brit. Jour. of Psych.*, **2**, 1907.

[6] *Brit. Jour. of Psych.*, **3**, 1909.

[7] "Zur Individual Psychologie," *Centralblatt f. Nerv. in Psychiatrie*, **14**, 1891.

[8] "Der Psychologische Versuch in der Psychiatrie," *Psych. Arb.*, **1**, 1896.

[9] "Experimentelle Studien über Associationen," *Psych. Arb.*, **1**, 1896.

[10] "Experimentelle Studien zur Individuellen Psychologie," *Psych. Arb.*, **1**, 1896.

[11] "Ueber die Messung der Auffassungsfähigkeit," *Psych. Arb.*, **2**, 1897.

Cohn in 1898,[12] Stern in 1900,[13] and Wiersma in 1902.[14] In these cases experiments, if made at all, were usually in the form of a few carefully prepared tests given to a few subjects either with a view to studying their individual variations in detail or else for the sake of discussing the question of method of administration. There is also the other method of work, that of testing large groups of school children, as for instance the work of Ebbinghaus in 1897,[15] Netschajeff in 1900,[16] Lobsien in 1901,[17] and Meumann in 1905.[18]

In France under the influence of Binet and his publications in *L'Année Psychologique,* there has been an enormous amount of work done, especially with children—investigations into normal and abnormal conditions, both mental and physical, culminating in 1905 and 1908 in the Binet and Simon sets of graded tests of intelligence adapted to children of all ages from three years up. In 1904, Toulouse, in his "Technique de Psychologie expérimentale," gave, as the result of nearly ten years' work, a full and detailed exposition of the methods of giving certain tests, and of computing the results gained.

In America following the publication in *Mind,* 1890, of "Mental Tests and Measurements" by Cattell with comments by Galton there was a rapid development of the work represented by that of Bolton in 1892,[19] Gilbert in 1893–94,[20] Shaw in 1896,[21] Griffing in 1896,[22] Macdonald in 1897–98,[23] Kirkpatrick in 1900,[24] Bagley in 1901,[25] Seashore in 1901,[26] Smedley in 1901,[27] Swift in 1903,[28] and others

[12] "Experimentelle Untersuchungen . . .," *Zeitschr. für Psych.,* **15**, 1897.

[13] "Ueber Psych. der Individuellen Differenzen."

[14] "Die Ebbinghausche Combinationsmethode," *Zeitschr. f. Psych.,* **30**, 1902.

[15] "Ueber eine neue Methode zur Prüfung geistiger Fähigkeiten und ihre Anwendung bei Schulkindern," *Zeitschr. f. Psych.,* **13**, 1897.

[16] "Exp. Untersuchungen über d. Gedächtnissentwickelung bei Schulkindern," *Zeitschr. f. Psych.,* **24**, 1900.

[17] "Exp. Untersuchungen über d. Gedächtnissentwickelung bei Schulkindern," *Zeitschr. f. Psych.,* **27**, 1901.

[18] "Intelligenzprüfungen an Kindern der Volksschule," *Die Exp. Päd.,* **1**, 1905.

[19] "The Growth of Memory in School Children," *Am. Jour. of Psych.,* **3**, 1892.

[20] *Studies from the Yale Psychological Laboratory,* **1**, **2**, 1892, 1893.

[21] *Ped. Sem.,* **4**, 1896.

[22] *Psych. Rev.,* **3**, 1896.

[23] "Experimental Study of Children," in *Report United States Comm. of Ed.,* 1898.

[24] *Psych. Rev.,* **7**, 1900.

[25] *Am. Jour. Psych.,* **12**, 1901.

[26] *Ed. Rev.,* **22**, 1901.

[27] *Report Dept. of Child-study,* **3**, 1900–01 (Chicago Public Schools).

[28] *Ped. Sem.,* **10**, 1903.

on school children; that of Jastrow in 1893,[29] Thompson in 1903,[30] and Ternan in 1906,[31] on laboratory subjects (in the last instance children who came to the laboratory regularly), and further work of Cattell in 1893[32]–96,[33] and Jastrow in 1893,[34] on college students. A study of method and a somewhat extended inventory of seven subjects has also been made by Sharp.[35]

Columbia appears to be the only university still making tests upon the freshmen. An inquiry among the universities and larger colleges of the United States and Canada has resulted in fifteen replies in the negative.

This by no means exhausts the list, since a large proportion of recent investigations of whatever topic include a treatment or statement of individual differences in method of work or degree of achievement, and since, too, some treatises on the psychology of individual differences, Stern's for example, are largely reviews of other investigators' general work from this particular standpoint.

There are, aside from the questionaire method so largely used by Stanley Hall and others by which large quantities of crude, descriptive material are amassed from untrained observers, two customary methods of experimental procedure which have already been indicated. One is to use a few specialized tests upon a limited number of subjects, with a sufficient number of repetitions to establish the reliability of the reaction or to induce fatigue or practise. Oehrn, Kraepelin, Ternan, Wimms, and Binet make use of this method. The second method, scoffed at by Stern and criticized by Binet in his review of Wissler's work, is to use very simple tests, many of them physical, upon large numbers of subjects, usually without repetition. Cattell's tests for freshmen, Galton's tests and the many tests of all kinds on school children are of this nature. This latter method is the predominant one in this country to-day.

That this should be the case, is not surprising since the first laboratory work directly concerning itself with individual psychology was instituted by Cattell whose early work in individual differences has been noted. Already in the eighties his experiments on himself and others[36] on the time taken to recognize colors, letters of the alphabet, to see and name the same, and on three groups of as-

[29] *Ed. Rev.*, **5**, 1893.

[30] "The Mental Traits of Sex."

[31] *Ped. Sem.*, **13**, 1906.

[32] *Phil. Rev.*, **2**, 1893.

[33] *Psych. Rev.*, **3**, 1896.

[34] *Am. Jour. Psych.*, **4**, 1893.

[35] *Am. Jour. Psych.*, **10**, 1899.

[36] "Psychometrische Untersuchungen," *Phil. Stud.*, **2**, **3**, 1895–6.

sociation tests anticipate much that has since become part of the regular stock in trade of those who use the methods of simple mental tests of the higher psychic processes. His list of ten tests employed upon all freshmen and other volunteers in the University of Pennsylvania published in 1890,[37] was the first definite psychological inventory in this country.

In 1896 following Baldwin's suggestion at the annual meeting of the American Psychological Association a committee of five was formed consisting of himself, Jastrow, Sanford, Witmer and Cattell to consider the feasibility of cooperation among the various psychological laboratories in the collecting of mental and physical statistics. A suggestive but indefinite report was made by this committee through Witmer the next year.

In 1907 the Association again appointed a committee of five consisting of Angell, Judd, Pillsbury, Woodworth, and Seashore to determine a series of group and individual tests with reference to practical applications, and to determine standard experiments of a more technical character. Their first report appeared in December, 1910.

Not the least interesting feature of the development of the work, has been the fluctuating of opinion with regard to its value, and the criticism of the methods used in accordance with the aim in view, and the evident influence of parallel work in general psychology. For instance in Germany there is first the intensive work on some of the higher mental processes by Kraepelin and his school in the early nineties, contemporaneously with extensive work in America on simpler processes with emphasis on the accompanying physical measurements—the subjects being sometimes children—and with characteristic French investigations into abnormal and criminal types as well as into the thinking powers of school children.

The long article in Volume 2 of *L'Année Psychologique,* 1895, by Binet and Henri, is notable in that it formulates two distinct problems of individual psychology, definitely favors the use of tests complex in content and therefore less capable of precise treatment, and suggests a grouping of appropriate tests under ten functions. In this article the preceding work of Cattell, Münsterberg, Jastrow, Kraepelin, and Gilbert is illustrated and criticized. The lists of tests given by the first three men are termed too simple, incomplete and too partial—that is confined too entirely to tests of memory, sensations and physical abilities. Kraepelin's are criticized as being not only partial but impractical since the tests require five hours for completion, necessitating several visits to the laboratory. Gilbert's

[37] *Mind,* **15**, 1890.

are said to show the difference in degree but not in kind between the thinking powers of the child and the adult. Their own list of tests could be given in from one to one and a half hours. In describing them only vague directions for administration are given, and occasional illustrative results from some tests already used with school children. They conclude by saying that their tests probably need modification, and might not disclose the finer mental differences between individuals similarly trained and belonging to the same social group. The work is fruitful in suggestions, though with a sketchy indefiniteness rather than a diagrammatic precision.

Further progress, especially in the application of the tests to school children, was made in each country but along lines already indicated. Ebbinghaus[38] devised and applied a new sort of test since known as his "combination" or completion test, which aroused no little interest and discussion.

In 1899 Sharp[39] took up the question of method. The first half of her work is largely a review of the theses of Binet and Henri, while the remainder is a careful study of some of the tests suggested by them, as applied to seven college students. She considers the results unsatisfactory except that they show that a single trial of any of the tests, made in the suggested hour and a half among single trials of many other tests, would be practically valueless and most unreliable, especially in the case of the tests of a complicated nature.

The following year appeared Stern's work, "Über die Psychologie der Individuellen Differenzen." This contains a review of methods, but not of results to date, and criticisms which are largely destructive. Thus in pointing out the dangers of extensity and the probable resulting superficiality, he makes some enlivening remarks on the American fondness for the questionaire method, comparing it to the questions concerning favorite author, color, food, etc., compiled in the autograph books of the *Backfisch* of the day, which results in what he elsewhere calls "pseudostatistics." He would place no reliance on the results of any series of tests which could be completed in an hour and a half, and considers the individual differences found in sensation and perception to be due to lack of experience with the material, since practise reduces those differences. He also says that tests on memory should seek to discover ways of memorizing and length of retention rather than content, and that as a measure of association, the spoken first idea is too erratic to be trustworthy, and measures too much else besides association. He offers few definite suggestions as to methods of procedure.

[38] *Zeitschrift für Psychologie*, **13**, 1897.

[39] *Op. cit.*

In 1901 Wissler, in working over the results of the Columbia freshmen tests from the point of view of correlation, finds so little that he concludes that they tell nothing as to the general intelligence of individual college students or adults. If a functional relationship exists it must be more complex than is usually supposed and it needs further testing. He remarks that correlating successive trials would help show the precision of a test.

Two years later appeared Binet's[40] account of careful and repeated tests, extending over several months, on his two little daughters. Methods and results are given in detail and the conclusions drawn from them as to the characteristics of the two subjects. Many of the twenty different tests were those already utilized in work among school children, notably the written descriptions of objects and pictures. His object was qualitative and descriptive rather than normative, and in consequence the actual tests are supplemented by long and careful questioning as regards imagery and analysis of associations.

The same year, in the introduction to the first volume of the "Beiträge zur Psychologie der Aussage," Stern again criticizes current methods of investigation. He points out that by them either time or numbers is sacrificed, whereas data from many people should be amassed by trained observers, and similarly treated. Instead of one experimenter using a few volunteer students as subjects, another large or selected groups of school children, another his own patients, another criminal cases, and still another results of a few experiments on himself and treated by original methods—the general results being confusion rather than cohesion—there should be an Institute for Applied Psychology, to act as a centralizing and unifying agency, a sort of clearing house, with the services of a trained statistician always available! The tests used should represent actual life conditions as nearly as possible and not be at all of the type of immediate memory for colors, tones, etc., which tell as much about the memory as a microscopic study of the finger would tell of its function. How well he has succeeded in justifying his position may be gathered from the successive volumes of the *Beiträge* and the *Zeitschrift für angewandte Psychologie.*

The next year a distinct advance towards synthesis and standardization of tests was made in the carefully prepared work of Toulouse, Vaschide, and Piéron.[41] Without quoting results to be expected or norms to be employed, explicit directions are given for the administration of nearly fifty tests, more than half of which are on memory.

[40] "L'étude expérimentale de l'intelligence," 1903.

[41] "Technique de Psychologie Expérimentale," 1904.

Ways of scoring are also illustrated at some length. The tests suggested have been selected from a wide and lengthy laboratory and clinical experience, and are, some of them, unduplicated in America, so far as I know. A condensed list will be given later. The methods of scoring too, do not seem so well known as Kraepelin's, for instance, perhaps because England and America are more apt to borrow from German than from French sources.*

There have been since then two types of test series in use, one of a simple nature useful in determining differences of large classes of people, the other of a more elaborate sort, applicable to a study of individual differences within a group, or to stages of development, or in some studies to the elucidation of the tests themselves. Thus epileptics, feeble minded, backward and truant children are studied as different from the normal type; twins, bright and dull children, younger and older children are compared, and individual differences in fatiguability by mental work, etc., investigated by the use of tests.

2. Representative Lists of Tests

By way of comparison some of the more representative lists are here given. They are not all complete, since the purely anthropometric tests have been omitted. It will be noted that a given test such as cancellation or tapping may be differently classified by different investigators.

Cattell's list, for students at Pennsylvania includes—

Rate of movement— of hand and arm through 50 cm.
Least noticeable difference in weight—lifted pairs (similar to Galton's test).

* After the experiments to be reported in this study had been made, there appeared Burt's article in the *British Journal of Psychology*, 1909, on ''Experimental Tests of General Intelligence'' and Whipple's ''Manual of Mental and Physical Tests.'' The former contains four new and interesting tests, and an elaborate treatment by the method of correlation. The latter is exactly what its title would indicate. Besides minute and explicit directions for administration and statistical interpretation of the fifty-four tests described, the published norms and extensive bibliographies are particularly helpful. The present study is a more specific attempt to determine relative values in the case of certain of the tests from which on the basis of general experience and a critical survey, Professor Whipple has chosen his standard series.

Finally there are now being published reports of the Committee on Tests of the American Psychological Association, which began its work in 1907. So far three studies have been reported: ''Methods for the Determination of the Intensity of Sound,'' by W. B. Pillsbury; ''The Measurement of Pitch Discrimination,'' by C. E. Seashore; ''The Determination of Mental Imagery,'' by J. R. Angell; all in Monograph Supplement No. 53 of the *Psychological Review*, December, 1910.

Reaction time for sound.	
Time for naming colors—	ten colors.
Space judgment—	bisection of a 50-mm. line.
Time judgment—	equate an interval to a 10-sec. standard.
Memory and attention—	number of letters correctly repeated after one auditory presentation.

JASTROW'S list for students at Wisconsin includes—

Rate of movement—	touching two reaction keys 38 inches apart in natural time.
	touching two keys 3 inches apart in quickest time.
Sense judgment—	estimate an ounce.
	equate two weights.
	estimate 1 inch on the skin.
	estimate position in guided movements.
	equate bilaterally symmetrical free movements.

JASTROW'S list for volunteer subjects at the World's Fair.

Sensibility, of touch—	distances in length.
	kinds of surface.
	weights.
of touch and sight—	bilateral symmetry.
	lengths.
	direction.
	location.
	aiming at a target.
of sight only—	lengths of lines.
	bisection, trisection, etc., of lines.
	number of letters, words, squares, colors, etc., seen in an exposure of 1/20 sec.
Memory—	visual immediate.
	recognition method for colors and forms.
Reaction time.	

This description of the list follows Binet's analysis.

GILBERT'S list for testing school children.

Muscle sense—	threshold for lifted weights.
Suggestibility—	size weight illusion.
Voluntary motor ability } Fatigue	rate of tapping.
Reaction time.	
Discrimination reaction.	
Memory of time.	

OEHRN'S list for 10 subjects.

Perception—	counting letters.
	proof reading.
	cancellation test.

Memory—	time to learn 12 nonsense syllables.
	time to learn 12 numbers.
Association—	adding one place numbers.
Motor—	speed of writing from dictation.
	speed of reading.

BINET AND HENRI'S suggested list.

Memory—	of a geometrical design.
	of 60-word sentences.
	of musical phrases.
	of colors (recognition method).
	number of repetitions needed to learn 12 numbers.
Images—	letter square.
Imagination—	questions as to tastes, etc.
	ink blots.
	suggestion from abstract words.
	coordination of a theme.
	completion of a drawing.
	construction of many sentences with given nouns or verbs.
	a ten-minute theme on a given subject.
	development of a musical theme.
Attention—	regularity of reaction times.
	reproduction several times of a line seen once.
	speed at which two metronomes at different rates can be counted.
	simultaneous reading and writing of different content.
Comprehension—	understanding of simple puzzle mechanisms.
	differentiation of synonyms.
	criticism of absurdities, fallacies.
Suggestibility—	an increase-in-length-of-line trap.
	discrimination of odors (odorless flasks).
	name and unannounced sensation from imposing-looking apparatus (none given).
	apprehension at second, slow trial of algometer.
	involuntary movements.
Æsthetic choice—	constancy in selection of rectangles, colors, etc.
	series of musical phrases.
Moral feelings—	kind of reaction to one photograph of brutal horrors included in a series of neutral scenes.
	behavior at a sudden loud noise.
Force—	dynamograph.
Motor skill—	(vaguely indicated) some form of maze test.
	throwing 10 balls at a target.

It will be noticed that the emphasis is on the qualitative rather

than the quantitative side, even in a series to be given at one sitting only. Following these suggestions, but with repeated sittings there is

SHARP'S list, used with seven subjects.

Memory—	immediate for 12 letters, visual.
	immediate for 12 numbers, visual.
	immediate for words, auditory, disconnected.
	immediate for sentences, short and long, auditory.
	for sounds, by question method.
Images—	letter square test.
	questions.
Imagination—	ink blots.
	puzzle watch and box.
	development of themes.
	questions on suggestions from abstract terms, etc.
Attention—	cancellation (in four variations).
	reading time of concrete and abstract material.
	simultaneous reading aloud and writing.
Observation—	description of picture exposed for 2 minutes.
	memory of colors exposed for 5 seconds.
	comparison of synonyms.
Tastes—	range of information about pictures.
	number of pieces of sculpture, artists, musical composers named in 5 minutes.
	naming one production of each of 10 composers.
	naming an author from hearing a selection read.

STERN'S suggested list.

Type of perception—	things highly colored named in 5 minutes (written).
	things of vivid sound named in 5 minutes (written).
	color recognition, after 10 minutes' interval.
	pitch discrimination with several minutes' interval.
	kind of mistakes in letter square test.
	reproduction of melodies and rhythms after several days' interval.
	estimate of location of a rotating hand on a dial after a given interval.
Memory—	time to learn lists.
	time to re-learn next day, noting accuracy.
	reproduction of an anecdote immediately, next day, a week, a month later.
Apperception type—	reproduction of a story.
	description of a picture, object, etc.

Attention—	distractibility during work from alteration in light.
	distractibility during work from interrupting sounds.
Combination (construction)—	formation of as many words as possible out of a given selection of letters.
Judgment—	suggestibility by weights, odors, changes in pitch.
Natural tempo—	constancy in rate on different days of beating a three-fold rhythm.

BINET'S list, used with his daughters.

Association and imagery—	writing a list of 20 words.
	first idea on auditory presentation of a word with many questions for introspection.
	writing sentences (time before beginning noted).
	completing sentences.
	developing a theme.
	writing down events recalled.
	description of objects.
	description of occurrences (pictures).
Attention—	cancellation test, varied.
	immediate memory of numbers heard.
	number of glances needed to copy figures and lines of prose.
	copying a drawing exposed .07 of a second, number of exposures needed.
	regularity and judgment of reaction time.
Memory—	amount of poetry learned in 10 minutes recalled immediately and 6 months later.
	immediate memory for unrelated words, auditory.
	immediate memory and description of objects seen.
	immediate memory for drawings of objects seen for 20 sec.
	immediate memory of hieroglyphs seen for 15 seconds.
Space and time perception—	reproduction in movement of a given length of line.
	equating an interval to varied standards.

TOULOUSE, VASCHIDE and PIÉRON list.

Memory—	visual, of colors, lines, angles, curves, location of dots in a circle, rates of movement.
	auditory, of tones, chords, arpeggio intervals.
	muscular, of lines, curves, positions.
	verbal, of numbers, letters, words, phrases (auditory).

	objects, pictures of.
	positions, jointed model of a human figure.
	sketches.
	musical, phrases, rhythms.
	logical, of a prose passage, auditory and visual.
	localization, grouped and serial order of 16 printed nouns.

(All the above to be studied by both reproduction and recognition methods.)

	time to learn long lists of numbers and letters, length of retention of lists.
	recognition of words in lists too long to have been learned.
	lists of words with prefix or suffix in common.
Attention—	cancellation test of letters, hieroglyphs.
	reaction time with discrimination, and irregular intervals.
Suggestibility—	algometer.
Perception type (objectivation)—	rate of tapping.
	reaction time to sight, sound, touch.
Association and imagery—	first idea, orally, from a starting word or object drawn.
	words with or without specified letters.
	associate or dissociate of a verb.
	free association, orally, for 30 seconds from a word or object drawn.
Imagination—	spelling words backwards, visual or auditory.
	giving syllables backwards, auditory.
	theme about a picture or drawing.
Abstract synthesis—	species-genus first idea.
Judgment and observation—	detection of absurdities, fallacies, etc. (oral presentation) and in drawings.
Reasoning—	completion of syllogisms.
	criticism of given syllogisms.

CATTELL'S *Columbia* freshmen tests. (* = discontinued.)

Sense discrimination and perception of space and time—	reproduction and bisection of a line.
	pitch discrimination.
	æsthesiometer.
	reproduction of regular rhythm.
	perception of weight (distance).
Memory—	numerals heard, immediate.
	numerals seen, immediate.
	logical, of a prose passage read aloud to them.
	retrospective, of line drawn and bisected, after 50 minutes' interval.
Imagery—	questions.
Motor—	ergometer.

	rate and accuracy of dotting.
	reaction time to sound.
	tremor in drawing a line.*
Perception—	reaction with discrimination.*
	cancellation test.
	naming 100 colors.
Association—	first idea, written.
	(opposites, written).
Æsthetic choice—	color liked and disliked of models shown.
Attention } *	
Apperception } *	
Suggestibility } *	

Whipple in his Manual[42] does not propose his list as one to be used in its entirety as an inventory of an individual, but would probably claim, and with much justice, that an adequate inventory would require his 54 tests or more and an expenditure of something like an equal number of hours. His list is not quoted, though it is the most important single contribution of the last decade to the topic, because it is readily accessible. It should be carefully studied by any one whose interests lead him to read the present report.

3. Aim of the Present Study

Without discussing the difference in aim revealed in the character of these series nor the results obtainable by the different methods, this study is concerned with only the usefulness of simple tests now employed or of similar tests designed to supplement or replace them because of greater significance or greater adaptability in content or method. With the exception of one or two association tests all are of the simplest type, and the question raised is, "If this kind of test is the sort frequently used, is it the best of its kind for the purpose?" To answer this adequately would necessitate collecting every simple test of intelligence known and experimenting with it from the points of view of make-up of the test, method of administration, results, change with practise, with maturity, with fatigue, etc.—too long and complicated a task for this study. By limiting the field, however, is caused the main defect of this work. If more of the time which has been spent over the statistics resulting from the data gained had been given in the first place to administering more tests of one function more carefully to more subjects there might be some definite value. Nevertheless, for such as it is, this study is now presented.

My best thanks are due to a friend who assisted in standardizing and correcting 360 pages of one of the cancellation tests, to the three friends who cheerfully served as subjects for so many hours in the

[42] *Op. cit.*

hot summer days of 1907, to N. who also helped in many of the later calculations, and lastly to Professor Thorndike for his ever ready counsel and patient assistance in the revision of both data and treatment.

In general this study is divided into two sections, one in which about 45 different tests repeated on from three to seven subjects are discussed from the point of view of correlation of the tests, change with short practise and reliability of a single trial, the other in which five very different tests practised with nine subjects are discussed from the point of view of change in each, and similarity of changes.

II

EXPERIMENTAL WORK WITH SEVERAL GROUPS OF TESTS

CONCERNING certain of the tests supposed to inventory an individual's mental functions and measure his differences from the type which are frequently given, as, for instance, the Columbia freshman tests,[43] we are still undecided as to their exact value. We need to know, (1) whether they test fundamental qualities slowly changing by general mental growth and the effects of training in general, or whether they measure degree of attainment in some specialized ability. If large areas of the mind are reached, then much might be predicted from them; if only narrow habits are tested, then little could be predicted from them. One line of evidence is their susceptibility to practise; for a test in which there is much change in a short period of practise is evidently measuring something other than a general function—it might be specialized ability, or the fact of becoming adjusted to test conditions, or the adoption of some device with regard to certain material.

We need to know, (2) in case general qualities can be measured by these tests, whether the test chosen is the best of its kind, the most typical. One line of evidence here is the correlation of different tests all supposed to measure the same thing.

We need to know, (3) how accurately the few trials made, often only one, will measure the function directly tested, how far, for instance, the result may be affected by the understanding of the subject of what he is to do and how he is to do it. The reliability of first trials can be worked out to give light here.

We need to know too, (4) how far results are influenced by differences in the method of administration. Can differences in attitude be made in the subject by varied direction of the attention? Practically the question is—"How could the tests now in use be improved in significance and accuracy?"

The methods at present in use with the students from Columbia and Barnard colleges must of necessity be more or less rough and ready, since only from fifty to sixty minutes are occupied in giving

[43] For a full list and descriptions of these, see Wissler, "The Correlation of Mental and Physical Tests," *Psych. Rev. Mon. Suppl.*, Vol. 3, No. 6, 1901.

some twenty to twenty-four tests; and in successive years they are given by different experimenters. Some of the subjects, particularly the girls, are too nervous to do themselves justice at the beginning of the hour, a fact which, as seniors, they frequently recall with amusement or deprecation. Comparison in such cases between performance as freshman and as senior will tend to overweight the gain shown in the results of the seniors' tests, and the consequent inferences as to the beneficial effect of college training.

The problems with which this first section deals are—

A. How far is each test susceptible to practise, especially to short practise?

B. What is the value of each test as a measure of the individual's ability in some general function or group of functions, such as memory, association or sensory discrimination?

C. How can we get the best possible measure from a single trial?

In general the procedure was as follows:

1. Three subjects, a highly selected group, made twenty trials of each of certain selected tests during six weeks in the summer of 1907. Of these three, N. had had comparatively little linguistic training, but, on the other hand, had exceptional preparation in psychology, particularly in giving tests similar to these. She was unusually quick in thinking and talking, also in writing and hand movements. W. and F. both had a more inclusive linguistic training, F. particularly so. Both had done graduate work in psychology, not including, however, much work of this nature. W. was somewhat variable in speed, F. was rather slower on the whole, with two notable exceptions, and was the least likely of the three to be put out or upset nervously. Conditions were made as uniform as possible during the tests, and record kept of the weather and temperature conditions from day to day. The association, perception, and memory tests were practised by the three subjects in a group. The discrimination and motor tests were practised by each separately, as individual attention and timing were necessary. The group work took about three quarters of an hour daily, the individual work from 20 to 30 minutes for each subject. The last two sets of trials were made under rather forced circumstances, as it became necessary to complete the twenty sets a little earlier than had been expected. The general trend of the practise curve was not affected however.

2. From experience with this group, called the "long-term-practise group" for convenience, certain of these tests, along with others supposed to be of a similar nature or to test the same mental process, were repeated in the spring of 1908 with a larger group of

subjects varying from six to eight members. These were junior or senior women students in Teachers College, four rather young, three rather more mature, and one man, some of whose records in the association tests had to be omitted owing to some difficulty with the English language. As much as possible was done with these subjects working in a group, for which purpose they met once a week for two hours for six weeks. They made from two to ten trials with different tests. Later, each came alone for work with some of the tests requiring special apparatus or individual attention. These subjects are referred to as the "short-term-practise group."

3. Certain random groups of college students were used either as opportunity offered or definitely in order to procure a larger number of control cases. One such group of nineteen summer session students spent an hour in 1908 in taking various association and perception tests; another group of similar size in the winter term spent half an hour on some of the tests. These have been called the "instructed group." Single tests are frequently given to large groups for demonstration purposes, and where available, these records have been utilized to get a standard average and deviation for maturer students working in a group. These are referred to as "control cases."

In discussing the work each test is taken separately and report made, first of general experience with the test, including the freshman results for men and women, then of the instructed group, men and women separately where so distinguished, next of the short-term-practise group, last of the long-term-practise group. Thus there is quoted first the result as found by the present test and method; next the results from more mature students, sometimes by a slightly different method; then the change taking place in naïve mature subjects with only a few repetitions; last, what change may take place even in habituated, mature subjects with more extended practise.

A test in which there is not much change will, other things being equal, be the more reliable to use for a single trial with naïve subjects. The "other things" must of course include ease with which directions are understood, simplicity of required reaction, and freedom from all pitfalls or traps for the well-intentioned but unwary subject.

For each group of tests the questions of change by practise, intercorrelation and precision are then taken up and recommendation made of one or another of the tests tested.

1. Tests on Association

A. *Descriptive*

The first group of tests to be reported on will be those on association.

The Columbia freshmen are given one test only, the *first idea,* the Barnard freshmen that and an *opposite* test.

First Idea.

This consists of the blank given below.

House
Tree
Child
Time
Art
London
Napoleon
Think
Red
Enough

(*N. B. This and many other blanks appear here in reduced size.*)

The test is explained to the students as one of rapidity in thinking rather than of quality. They are told to write as quickly as possible after each word the first idea—preferably one word—that occurs to them. Practise is given orally with a sample word, then the students are handed the blank. The time taken to finish the blank is taken on a stop-watch, and the blank is filed.

One's common observation in giving this test to the freshmen is that it is particularly hard to follow the directions, and to write down actually the first idea that occurs on reading the word. Subjects will sit blankly, stopped by a word, obviously choosing the fittest of several ideas, however well it may have been explained to them that it is primarily a test of the rate rather than of the quality of thinking. The averages calculated from 250 Columbia and 100 Barnard freshmen show that the men take 55.4 seconds to write down 10 ideas, the girls 71.8 seconds. The P.E. for Columbia students is 22.9, quite the largest P.E. found for any of the freshman tests. To make these figures easily comparable with those to be given for subjects after short and long practise, they may be put thus: in 15 seconds men, as tested in the regular manner, wrote 2.7 first ideas, girls wrote 2.1 first ideas, or the average time to call up and write one idea is 5.54 seconds for men and 7.18 seconds for women. In this test then, the girls seem specially hampered; for the results of other tests of the rate of association, such as adding, and giving the opposites of words show no such superior speed for males.

The method used in the present investigation was to explain very carefully just what was wanted, giving oral practise with two sample words. Subjects were told to begin at the signal "go" and get as much as they could done till the signal "stop" was given. They were warned that they would not have much time, though the actual number of seconds was not told them in advance. (The three subjects who took the long term of practise soon came to know the time allowed for the different tests.) For the *first idea* test, the time-limit was 15 seconds. The score was kept in number of words written. Three letters counted as a word if the subject could explain that he had surely thought of something.

A single trial with 37 unpractised subjects, 19 men and 18 women, with the time-limit of 15 seconds gave an average of 5.6 words written, with an average deviation of 2.19 or an average of 2.68 seconds to call up and write a word. The men and women had exactly the same average, but the A.D. for the men was 2.58, for the women 1.78. Unless then, the apparent sex difference in the freshman results is due to difference in the relative immaturity of the subjects, it may be produced by the method of giving the test. (For convenience, the method by which a subject is told to work as quickly as possible and the time taken to finish the test is noted will be called the "amount-limit" and the method by which the subject starts and stops at a given signal, and a certain time-limit unknown to the subject beforehand is allowed, will be referred to as the "time-limit" method. The latter has obvious conveniences in testing groups of subjects.) In each test where both methods were used, comparison will be made of the results by each method, and a special section devoted later to a summing up of these results.

By the amount-limit method 2.7 first ideas were written in 15 seconds by the men, by the time-limit method 5.6; by the women the averages are 2.1 and 5.6 respectively. These differences suggest first, that the amount-limit method leaves the test ambiguous, the time being a measure partly of slowness in associations and partly of associations called up and rejected; second, that a time-limit acts as a spur, making subjects work more quickly than if simply directed to write as quickly as possible, and making them less fastidious in selection of associations when speed is so much emphasized. It is known that "controlled association time" is often shorter than free association time, the theory being that the setting of the attention and judgment beforehand holds certain paths open for use more readily than others; it may be then that attention is aided in a somewhat analogous fashion by the incentive to do as much as pos-

sible in a given time. The anticipation of the signal "stop" seems to give a more definite aim than merely one's best effort after speed.

TABLE I

		Words written in 15 seconds		Seconds required per word	
		Men	Women	Men	Women
Amount limit		2.7	2.1	5.54	7.18
Time limit					
Instructed		5.6	5.6	2.68	2.68
reversed			4.6		3.26
Short	1st		7.0		2.14
	Average		7.85		1.91
	4th		8.2		1.83
Long	1st		7.0		2.14
	Average		7.83		1.92
	20th		8.6		1.75

It was, however, suggested, that the list of words as printed lent itself to higher scores by the time-limit method than by the amount-limit, as the more concrete words come near the beginning, and the most difficult are the three last. To test this point, the list was typewritten in reverse order and then used as a time-limit test with two other groups of students, 29 rather young women, and 34 in a mixed group of men and women somewhat older. The average number written in 15 seconds was 4.6 words. Asked to repeat the test commencing with the bottom word, the average in 15 seconds was 4.8 words. Thus the greater speed does not seem to be entirely due to the kind of words encountered at the outset.

In the short term of practise, 4 trials on different days by 6 subjects by the time-limit method, the average was 7.85 first ideas written in 15 seconds, or 1.91 seconds per word. In the long term of practise, 20 trials by 3 subjects, the average was 7.83, or 1.92 seconds per word. The number written at the first trial by each group was 7.0. Taking all the trials of these two groups into account, 85 in all, there were 14 occasions, or 16 per cent. of the total number, when the test was completed in 15 seconds. The two lowest records, made only once each, were 3 and 5 first ideas, both considerably higher than the freshman results by the amount-limit method.

The difference appears even more striking when the fairly constant factor of speed of writing is discounted. Three subjects were given six trials each in writing ten words of some familiar sentence* under each other in a vertical column. The average time for the 18

* Two clauses from the Lord's Prayer: (1) Our Father, etc.; (2) Lead us, etc.; and (3) "Little Jack Horner sat in corner eating his Christmas pie." The number of letters were 40, 43, and 48.

trials was 13.38 seconds or 1.34 seconds a word. Thirty subjects, naïve except for an hour's work in other tests, were asked to write a single word similarly with a time limit first of 10 seconds, then of 15 seconds. Half of them wrote the word "watch" in the 10-second test, the word "father" in the 15-second test; the other half wrote "father" in the first test, "watch" in the second. The results were for the 10-second test 5.1 words, for the 15-second test 7.75 words, or an average time of 1.95 a word or .355 second a letter. Thus the average extra time needed for association over mere writing is, in the case of the amount-limit method, about five seconds a word; in the case of the time-limit method less than 1 second a word.

In absolutely free association—*i. e.*, when a starting word only was given and the subjects wrote down whatever series of things they thought of, an average of 11.5 words was written in 15 seconds, or at the rate of 1.31 seconds a word. (Incidentally it is interesting to note that serial connections are more rapidly written than even the same word in repetition, thus:

Familiar sentences, 3 subjects, 18 trials, 1.34 seconds per word, .307 per letter.
Free association, 6 subjects, 30 trials, 1.31 seconds per word, .240 per letter.
"Father" or "Watch," 30 subjects, 60 trials, 1.95 seconds per word, .355 per letter.

though this difference is partly due to the fact that the 18 trials came from 3 practised subjects on different days, the 30 trials from 6 subjects after the short term of practise, the 60 trials from 30 subjects after 1 hour's work with various tests.)

It seems certain then that the *first idea* test, as usually given, does not measure the rate of association. Nor apparently can any test involving the writing of words do so. For not only is the average rate of mere writing no less per letter than the average rate of writing words under some associative requirement, but in certain cases where the description of the association involves writing a phrase or long word such as "eyes, nose and mouth," "kerosene oil" or "pussy-willow," the writing time entirely obscures the association-time.

Considering it from the point of view of practise, in the short irregular practise with the average score of 7.85 the fourth trial showed a gain of 1.17 or 17 per cent. over the first. With the three subjects who repeated the test twenty times there was a practise gain of 1.6 or 23 per cent.

In the five trials with the absolutely free association test there was quite the reverse of practise effect. The starting words used at

the five trials were, respectively, *house, read, black, table, ball.* The average amount done in 15 seconds was 11.5 words, or one word in 1.31 seconds, the deviation of the first trial from this 11.5 being + 1.8, of the fifth — .8.

The correlation of the *first idea* test with other association tests will be taken up later.

Opposites Test.

In giving this test the usual experience is that some words are uniformly hard, and that when once at a loss for the opposite to any word that has presented difficulty, an enormous amount of time may be spent. Some subjects will go on writing the easier ones, returning afterwards to those that have proved puzzling. If these have been retained subconsciously there is probably a saving of time. Usually no hint is offered about ''skipping'' in this way to the freshmen, though where this test has been used in group work with children and others, with a time limit, usually no skipping is allowed. It then becomes impossible to know how much of the time is spent over perhaps one word in the list, so that the final record is very much affected by the inherent difficulty of the test-words. The standard set prepared by Woodworth and Wells* is not in common use yet, and the Columbia set presents several difficulties. It is as follows:

Write as quickly as you can beside each word in the column a word which means the opposite thing from it.

barbarous
simple
rude
obscure
gentle
to expand
elation
adroit
loquacious
to degrade
to hinder
precise
permanent
repulsion
to respect
genuine
separate
deceitful
grand

* To be reported as a publication of the American Psychological Association.

Other sets used in comparison were:

Opposites Tests

I

day	vertical	right	good
asleep	to spend	love	outside
absent	to reveal	rude	quick
brother	level	just	tall
best	ignorant	lie	big
above	past	tidy	loud
big	part	cruel	white
backwards	motion	run away	light
buy	to hold	best	happy
come	generous	quick	false
cheap	proud	remember	like
broad	diligent	dressed	rich
dead	stupid	to be hit	sick
land	serious	lose	glad
country	frequently	mend	thin
tall	weary	disobey	empty
son	wicked	clean	war
here	to create	noisy	many
less	to enrage	rough	above
mine	stormy	cross	friend

			II
serious	high	great	vertical
grand	up	hot	ignorant
clumsy	wet	dirty	rude
to win	new	heavy	simple
to respect	soft	late	deceitful
frequently	wider	first	stingy
to lack	wrong	left	permanent
apart	yes	morning	over
stormy	young	much	to degrade
motion	laugh	near	weary
forcible	winter	north	to spend
to float	weak	open	to reveal
straight	forget	round	genuine
to hold	wild	sharp	level
after	beginning	east	broken
unless	straight	known	wild
rough	raise	something	part
to bless	rough	stay	past
to take	love	push	permit
exciting	noisy	nowhere	precise

In scoring these, a mark of 2 was given for the best choice, 1 for a second best choice, and 0 for a bad choice. The key used in scoring will be found, alphabetically arranged, in the appendix. From the very fact that so many words could be offered as opposites to certain

given words, it will be seen how valuable a standardized set would be. In the various tables that follow a score for accuracy is given in terms of the per cent. which the score given to the individual in question was of the score he would have received had every opposite written by him been rated as worth 2 credits. Thus a record of five opposites valued as 2, 0, 1, 1, 2 respectively is scored 6/10 or 60 per cent.

First, to compare the various blanks used. Columbia freshmen have not been put through this test. Barnard freshmen have usually taken the "barbarous" blank, though 14 were given "vertical I." "Barbarous" took 166 seconds on the average or 8.74 seconds per word compared with 105 seconds, or 5.25 seconds per word for "vertical I"; the scores for accuracy were (average) 69 per cent. and 72 per cent. respectively. The short-term practise group who also worked with each blank, and by the same method, took 141 seconds, or 7.42 seconds per word for "barbarous," and 89 seconds, or 4.45 seconds per word for "vertical I." Their average scores were 69 per cent. and 71 per cent. Thus the difference in time taken shows that the "barbarous" blank is more difficult than "vertical I." The average score for "barbarous" is also lower than that for any other blank, as may be seen from Tables II and III. An easier blank, such as "serious" or "day" would probably be more suitable for this type of subjects.

TABLE II

SPEED AND ACCURACY IN WRITING OPPOSITES

	"Barbarous"			"Vertical I"			"Vertical II"		
	Time required in seconds for 19 words	Seconds required per word	Av. per cent. of max. credit obtained	Time required in seconds for 20 words	Seconds per word	Av. per cent.	Time required in seconds for 20 words	Seconds required per word	Av. per cent.
Freshmen	166	8.74	69	105	5.25	72			
Seniors	93	4.89	69				96.2	4.81	71
Short-term ...	141	7.42	69	89	4.45	71			

So far as these blanks reveal differences in maturity, there is a decided improvement in speed with more mature subjects; the freshmen take a longer time than the short-term group at their first trial with both the difficult blanks, and considerably longer than the seniors. The accuracy is practically the same for all these three groups on the same blanks. Looking also at Table III, all the records from the short-term group are poorer than even the first record of the more mature long-term group for "vertical II" which is a

fairly difficult blank, though the easier blank "day" seems too easy to show differences in the groups of subjects. In this table all the records are reduced to the amount done in 30 seconds, and the accuracy score to percentage, whether the test was by amount-limit or time-limit method, and no matter what the blank.

To compare differences in method, a group of Barnard seniors were given "vertical II" by the amount-limit method, and a group of Teachers College women students the same blank by the time-limit method, with scarcely any difference in the results, though what there was, was in favor of the time-limit method, as will be seen by Table III. These two groups were of about the same maturity, but again with the slight difference in favor of the Teachers College students, so that either this factor, or that of difference in method may be responsible for the very slight difference in the figures.

TABLE III

SPEED AND ACCURACY IN WRITING THE OPPOSITES OF GIVEN WORDS

Speed is measured by the number of seconds required per word. Accuracy is measured by the average per cent. of the maximum credit that was obtained.

	"Barbarous" Test		"Vertical I" Test		"Vertical II" Test		"Serious" Test		"Day" Test	
	Speed	Accuracy	Speed	Accuracy	Speed	Accuracy	Speed	Accuracy	Speed	Accuracy
Amount limit										
Freshmen	8.74	69	5.25	72						
Seniors	4.89	69			4.81	71				
Short term	7.42	69	4.45	71						
Time limit										
Instructed					4.62	73			2.36	93
Short term 1st					4.48	70			2.21	91
Short term last					4.55	75			2.03	94
Long term 1st					3.23	91	3.13	86	2.50	94
Long term Average					2.48	88	2.22	88	2.19	95
Long term 10th trial					2.17	89	1.76	90	2.07	94

To test the effect of practise, the short-term group were given six different tests, the "day" being repeated after six weeks, giving 7 trials in all with the time-limit of 30 seconds, also "vertical II" once with a time-limit of 30 seconds. The Columbia blanks were given on the fifth day by the amount-limit method, so that a total of 10 trials was made by this group of subjects.

Since the "day" test when repeated after practise with "good," "great," "vertical," and "right" shows so little gain the practise effect is very slight, and the test continues to be an association test rather than a series of specially trained responses.

Even special practise with the same blank shows rather slow im-

provement. The long-term group used three blanks only, "day," "serious," and "vertical II." After the first two trials these were used in rotation till it was evident that the easy "day" blank had been memorized. The other two were used ten times each, on alternate days, and beginning alternately at the top and the bottom of the column. There was, of course, a gain in speed, the time per word being reduced from 3.23 to 2.17 and from 3.13 to 1.76 in the 10 trials, but the rate is still much above that for writing the numbers from one to twenty or other familiar series.

Comparing this test with the *first idea* in rapidity, it will be seen that this form of controlled association does take slightly longer with subjects practised with both tests.

TABLE IV

Seconds Required per Word to Write (1) The First Idea Called up by a Printed Word, (2) A Series of Words Started by a Printed Word, and (3) The Opposites of the Words of the "Day" Blank

	(1)	(2)	(3)
Time limit			
Instructed group	2.68	1.31	2.36
Short-term group	1.91		2.11
Long-term group	1.92		2.19

Other controlled-association tests used in comparison with this were: for the "instructed" group, two in number, the *preceding letter*, and *complete the word;* for the "long-term" group, six in number, these two and also the *subject predicate, difference between, Ebbinghaus combination,* and *addition;* for the "short-term" group, the first five given above, a different set of *addition and subtraction, noun and adjective, nonsense words,* and one or two *nonsense sentences, genus species, multiplication.* They will be taken up in that order.

Except where otherwise stated, these were always given by the time-limit method.

Preceding Letter.

The series of stimulus letters is as follows:

f
k
s
p
w
l
e
r
d

o
v
j
n
t
h

The time-limit was 15 seconds. The subjects were told to "write beside each letter the letter which precedes it in the alphabet," oral examples being given by two letters. With 197 subjects, one trial, the average number written was 5.5 letters, a clear mode of 5, a range of from 0 to 12 and an average deviation from the mode of 1.6. One letter thus required 2.73 seconds (Av.) or 3 seconds (Mode). Introspective evidence shows that this is a peculiarly difficult test to start right in spite of the preliminary oral practise. Old habit asserts itself to such an extent that many subjects are unable to react at all without mentally repeating the whole of the alphabet up to the test letter. Others try to repeat it backwards; others to make use of visual imagery. If this is the first test given in an hour's work on various tests, it seems particularly bad. When it is the sixth or seventh test given, the average on three different occasions with small groups, making 36 subjects in all, was 6.1 letters in the 15 seconds, or 2.46 seconds per letter, with an A.D. of 1.2.

The short-term group used it three times with an average of 7.3, the first day's average, 5.6, deviating by — 1.7, the last by + 1.0, showing a very decided practise effect for so few trials. The long-term group made averages of 7.3 letters or 2.05 seconds per letter, 6.3, or 2.05 seconds per letter, 8.6, or 1.74 seconds per letter, and 9.3, or 1.61 seconds per letter, in their first four trials. They were also very variable throughout the entire 20 trials. This test then seems to be a specially bad one.

Complete the Word.

The form of the test was as follows:

1. ri	11. med
2. bon	12. bus
3. mil	13. spo
4. la	14. gam
5. flo	15. an
6. chi	16. che
7. dr	17. chu
8. fas	18. we
9. sk	19. rec
10. bra	20. par
	21. chap

Fifteen seconds was allowed. Eight subjects used it three times, and the three subjects ten times, beginning with the first or second column or at the end, after which they made ten more trials with fresh sets.

In a first trial it is very noticeable that a subject may think of long words in the beginning, and continue to think of them even when shorter words are completed in the spelling out of the word actually written, as "ri" suggesting "ribbon" when "rib" would suffice, or when cognates would be shorter, such as rite for ritual. At the same time it is introspectively an easier test than the *first idea,* because, in the first place, the subject seems to be less suspicious of what may be demanded of him, and feels more free to write down what he has actually thought of; in the second place, parts of words seem to be more suggestive of whole words than one word is of another, perhaps for two reasons; first the conditions are more like ordinary reading, second the motor or auditory imagery or perhaps the incipient movements of the speech organs seem to perform the task of completion automatically, while all the judgment has to do is to acquiesce. With both this and the absolutely free association test, the factor of long words may increase the time taken through the mere mechanics of writing. The statistical results will favor those who think of short words as well as the rapid thinkers.

For the "instructed" group of 37 subjects the average number of words completed in 15 seconds was 8 (1.88 seconds per word), with a range of from 3 to 15, and an A.D. of 2.8.

TABLE V

NUMBER OF WORDS COMPLETED IN 15 SECONDS

	No. of subj. Men	No. of subj. Women	No. of trials	Av. No. written Men	Av. No. written Women	A. D.	Sec. req. per word Men	Sec. req. per word Women
Instructed group	19	18	1	8.2	7.7	2.8	1.83	1.94
Short-term group (using the same blank):								
1st					9.5			1.58
average		7	3		9.1	2.0		1.65
last					11.4			1.31
Long-term group (using different blanks):								
1st					9.3			1.61
average		3	10		10.5	.8		1.43
last					11.8			1.27

The short-term practise group in three trials made an average of 9.1 words completed or 1.65 seconds per word, with a range of from 4 to 15 and an A.D. of 2.

The long-term practise group averaged 10.6 words in 15 seconds or 1.42 seconds per word in their first trial. After 10 trials with the

same blank, improvement being very rapid, 10 more trials were made, with two or three from the original blank introduced into each set. The average was then 10.5, ranging from 9.3 on the eleventh day to 11.8 on the twentieth, showing a slight practise effect. Had the word beginnings been absolutely new, the practise effect would presumably have been still less.

Six of the short-term practise group later took this test orally by the amount-limit method. Eight trials were made with different lists. In this way it could be seen how a poor record is made by the influence of some one combination which halts a subject unduly long rather than by slowness in general. One list seemed easy for all subjects, but no one list was hard for all subjects; one or two exceptionally poor records occurred with every list. The combination "um" halted three subjects a comparatively long time. One subject made the worst record 7 times out of the 8, though in the written test by the time-limit method she had been one of the best subjects. Introspectively, all preferred the oral method. Compared with other tests, *completing words* is less disturbing than the *first idea*, but less definite than the *opposites*.

Subject-predicate.

As a test this is not in common use, so that the blanks were prepared in round handwriting, which may have retarded the speed somewhat as compared with the *first idea* and *opposites* tests, which were printed. Mimeographed sets were later used for the short-term practise group.

SUBJECT-PREDICATE LISTS

convenes	matriculates	stings	brays	confesses
butts	scratches	parries	steals	lubricates
explodes	earns	waxes	preaches	hatches
hops	bleats	prescribes	plays	disperses
sucks	illuminates	swims	arrests	reverberates
plants	paints	enlists	lectures	hoards
chases	flies	buys	flashes	smoulders
alleviates	experiments	quacks	rings	ordains
extinguishes	strikes	applauds	fights	nourishes
re-acts	reaps	sews	condemns	sneers
ebbs	cackles	navigates	graduates	performs
composes	inherits	freezes	burns	sells
shoots	learns	riots	drives	amputates
bites	blows	sues	cleanses	neighs
stitches	testifies	disbands	crows	rotates
trumps	owes	governs	calculates	fades
shines	adjourns	roars	haunts	bets
hammers	sings	occurs	melts	tolls
marries	sacrifices	raves	limps	foretells
trots	flows	surrenders	withers	barks

Subjects were warned not to supply a subject by forming a noun in "er" from the verb such as "sing*er*" sings, nor by using indefinite words as "man," "boy," but to supply the definite agent such as "bird." Two or three examples were illustrated. One hundred verbs were made up in ten sets of ten, each being used twice for the long term of practise, and once each on typewritten sheets for the short term of practise. Unfortunately for strict comparison they were not given in the same order for the short practise as for the long. The scoring for accuracy was done as for the *opposites* test, giving 2 for the best choice, 1 for a poorer one, 0 for a poor one.

TABLE VI

N. = number of subjects written to fit given predicates in 20 seconds.
Acc. = per cent. of maximum credits obtained.

Order given	1		2		3		4		5		6		7		8		9		10	
Tests	confesses		ebbs		cackles		navigates		brays		convenes		graduates		performs		stings		matricula	
Subjects	N.	Acc.	N.	Acc.	N.	Acc.	N.	Acc.	N.	Acc.	N.	Acc.	N.	Acc.	N.	Acc.	N.	Acc.	N.	A
Bu.	10	75	9	100	9	89	6	92	8	100	8	99	6	92	9	100	4	75	10	
Gr.	2	25	6	100	7	71	2	100	5	100	5	90	6	100	7	64	4	75	10	
J.	4	63	6	67	7	71	6	83	8	88	6	50	7	86	4	100	5	30	7	
L.	5	70	6	100	7	79	7	71	5	100	4	63	5	80	7	64	6	92	7	
M.	5	30	6	100	5	60	3	33	5	100	4	100	3	100	8	88	5	50	7	
Ba.	10	65	9	44	9	33	8	75	9	89	7	86	8	63	10	55	10	30	10	
Bf.									8	100	5	80	7	100	8	100	8	69	8	1
Averages .	6		7		7.3		5.3		6.8		5.5		6.0		7.5		6		8.4	
Medians ..		64		100		71		79		100		86		92		88		69		

TABLE VII

N. = number of subjects written in 20 seconds to fit given predicates.
Acc. = per cent. of maximum credits obtained.

	First trials 1–10		Second trials 11–20	
	N. Av.	Acc. Median	N. Av.	Acc. Median
performs	5.6	100	9.1	100
stings	7.0	94	7.1	93
matriculates	6.6	93	7.1	100
ebbs	7.0	94	8.0	94
brays	8.3	95	8.8	100
cackles	7.6	94	7.3	100
convenes	5.6	100	8.0	88
navigates	7.0	81	8.6	94
graduates	6.3	93	7.1	100
confesses	8.8	100	8.6	95
Average	7.0	95	8.0	96

The results for the short-term group are shown in Table VI. The practise effect is apparently very slight, the last five tests being only a trifle better in speed or accuracy. Further tests are, however, needed to separate the influence of differences of the tests in diffi-

culty from that of practise, and from that of the chance variations in the subjects.

The results for the long-term group are summarized in Table VII. The practise effect of ten trials, including one of the same blank, is in general to increase the speed only by a seventh, leaving the accuracy uninfluenced.

The time required in these tests is about the same as that in the difficult "vertical" opposite test.

The "Difference Between."

The form of the test used is as follows:

Answer these questions as quickly and as well as you can.

1. What is the difference between *grab* and *take?*
2. What is the difference between *eat* and *devour?*
3. What is the difference between a *stream* and a *river?*
4. What is the difference between a *wagon* and a *cart?*
5. What is the difference between *sorry* and *sad?*
6. What is the difference between *naughty* and *bad?*
7. What is the difference between *homely* and *ugly?*
8. What is the difference between *right* and *correct?*

Other lists used were:

II

confess, reveal
confine, limit
colleague, partner
bend, curve
resistance, opposition
deceive, mislead
adrift, afloat
extend, increase

III

above, over
demonstrate, illustrate
deluge, flood
guardian, keeper
merry, gay
bring, fetch
heavy, weighty
innocent, harmless

IV

show, indicate
watch, observe
trial, test
contract, bargain
peace, repose
clear, obvious
cleanse, purify
classify, arrange

V

get, provide
win, gain
pair, two
parcel, bundle
womanish, feminine
put, place
boat, ship
clever, talented

VI

chuckle, giggle
honest, honorable
procure, obtain
haste, hurry
crayon, chalk
antagonist, opponent
puff, swell
abrupt, blunt

VII

walk, march
ignore, overlook
corpse, carcass
early, soon
allude, refer
drag, pull

VIII

walk, march
deceive, mislead
corpse, carcass
colleague, partner
drag, pull
adrift, afloat
try, test
extend, increase

The subjects were told that the quickest way to answer was either to explain one word in terms of the other, or to write 1 = —— 2 = ——, not wasting time by repetition. Notwithstanding this, many to whom it was given used an unnecessary number of words in explanation, thus taking longer to write. From the point of view of time consumed, then, it is not a useful nor a satisfactory test whether given by the time-limit or by the amount-limit method. Not only association and speed of writing enter in, but the ability to profit by the advice in the instructions, and ability to condense—also, of course, linguistic discrimination. This test is, besides, not very easy to score, as the answers may vary considerably.

Blank I was kindly filled in at leisure by one of the professors in the English department. Answers were then compared with these standard answers and each of the eight scored 2, 1 or 0, as in the case of the *opposites* and *subject-predicate* tests. For the remaining blanks, dictionaries and books of synonyms were resorted to for standard answers, or, failing anything sufficiently discriminating there, the experimenter's own judgment of the best answer in the group was followed.

An "instructed" group of about 200 were tested with Blank I, time-limit of 120 seconds. In 49 of these chosen at random the average number of answers written was 4.4, with an A.D. of 1.08 and a range of 2 to 8. The average score for accuracy was 89 per cent. (reliability 1).

The short-term practise group took this test only twice, using Blanks I and VIII. The reason more time was not spent with them on the various blanks was that previous experience with the long-term practise group seemed to indicate that the test was not a valuable one. For the same reason and also because the 49 control cases from the "instructed" group were in terms of time-limit, this group were tested by the amount-limit method. Their record for Blank I was: average time taken 217 seconds, score for accuracy 73 per cent.; for Blank II, 233 seconds, score for accuracy 63 per cent.; for both blanks together, average time taken, 225 seconds, A.D. 25.5, average

score 68 per cent. For them, then, Blank I was easier since they made a better showing with it, although it was the first one given.

An "instructed" group of 49, tested with Blank I, with a time-limit of 120 seconds, averaged 4.4 answers written, A.D., 1.08. The average accuracy was 89 per cent.

The long-term practise group used seven different blanks altogether, each one three times except the last, beginning with the 1st, 3d, or last of the 8 pairs of terms. A time-limit of 60 seconds was allowed. Their average for Blank I was 4.6, score of 66 per cent. The average number written for all 20 trials was 3.2, the first day's average deviating by + 1.4, the last by + .4. The average score for accuracy was 70 per cent., the first day's average deviating by + 6 per cent., the last by + 3 per cent. Thus the difference in the difficulty of the blanks again disguises any practise effect. If the records of the first three trials which were made with Blank I are omitted, the average number written is 2.7, the fourth day's average deviating by — .7, the last by + .9, so that there seems a slight gain in speed. The average score for accuracy is then 77 per cent., the fourth day's average deviating by — 2 per cent., the last by — 4 per cent.

Nothing can be surely inferred from these records save that for them less than 20 seconds sufficed to think of and write out a difference (only 13.1 seconds for Blank I). A much longer time limit should have been given.

On the whole, as will appear when the facts concerning correlations and reliabilities are given, this test, if useful at all, is useful only as a specialized measure of linguistic knowledge and facility in expression. The times 27.3 seconds per difference for 49 subjects using Blank I, 27.1 seconds per difference for 6 subjects using Blanks I and VIII, and 18.8 seconds for 3 subjects using Blanks I–VII, show that an elaborate process of selective thinking is involved.

Ebbinghaus Combination Test.

This test was as follows. For the short-term group certain paragraphs of convenient length, averaging 100 words, were chosen from such varied materials as newspaper reports, scientific articles, essays, novels, narrative poems. These were typewritten, with 10 to 16 words, according to the length of the paragraph, omitted in various places, blank spaces being left in their stead. One such paragraph was placed before the subject, who was instructed to write down an appropriate word for each space. The time taken was noted, and a score was made of the suitability of the words supplied in terms of per cent. of a perfect record. Five of the short-term practise group

took ten such tests, repeating the first paragraph used at the 10th trial three weeks later.

In general, subjects will either skim two thirds to the whole of the paragraph at the outset, going back to fill in the spaces, or they will rush at the first phrase, fill in the first thing that occurs, and get tangled up before the end of the first sentence unless the subject matter is very easy. From one or two such experiences the subject is generally led to adopt the other method.

The short-term group took an average of 103 seconds to complete a paragraph, with an A.D. of 32. Comparing their two trials (three weeks apart) with the same paragraph there was an improvement in average speed from 173 seconds to 71 seconds, the A.D.'s 33 and 6 respectively. Their accuracy rose from 70 per cent. to 80 per cent. or, omitting one subject who seemed very much upset at the first trial, it was 80 per cent. on both occasions.

The long-term group was tested with 20 paragraphs averaging 92 words long, each with ten words omitted; they averaged 80.2 seconds, A.D. 18 seconds. Variations of 10 per cent. or less in the length of the passage caused no appreciable differences in the time required. Variations in the content are very influential. The poetry was difficult for these subjects, the average time for that being 108 seconds. Newspaper reports were easy, the average time for them being only 54.4 seconds. Picking the first trial of each kind of material, and comparing it with the last of each, there was an improvement in speed from an average of 104 seconds to 89 seconds. These figures do not measure practise with surety, owing to possible variations in the difficulty of even the same kind of material. The average accuracy was 87 per cent. with no discoverable practise effect. The paragraphs they used are given in the appendix.

In general it appears that adaptation to the form of problem set by the Ebbinghaus test is likely to count considerably, especially with untrained subjects.

Addition.—The blank used was as follows:

Addition Examples

17	26	27	72	23
42	51	24	14	47
38	47	83	39	86
91	82	19	81	54
54	63	45	26	36
17	42	38	91	36
26	51	47	82	26
27	24	83	19	45
72	14	39	62	63
23	47	86	54	54

41	53	67	78	86
52	67	86	37	32
86	34	23	96	44
23	78	45	72	36
35	19	67	23	68
45	52	19	45	23
13	86	78	67	72
68	23	67	78	36
77	35	23	37	68
86	67	86	96	39

A score of 1 for each column added was given and 0.5 deducted for each wrong figure in an answer. The time limit was 60 seconds. The results as to rate will be discussed in connection with those of the next test. Since these experiments were made, it has been shown by Wells and Thorndike that even so familiar a process is, under test conditions, subject to adaptation and practise effects. In these subjects these effects were shown chiefly or wholly in the speed of the process. The short-term group averaged 16, 19, and 18 columns, and .5, .67, and 1.33 errors in three trials on February 15, March 7, and March 7. The long-term group gained in twenty trials about 20 per cent. in speed but lost somewhat in accuracy, so that their net improvement was 17 per cent.

Addition and Subtraction.

The short-term group used a blank, given on the next page, from the collection prepared by Woodworth and Wells.

The test consists of adding a certain number to each figure in succession in the column, or subtracting it, as directed, and writing down the result. One column was counted as a test, making 25 times that a given number was added or subtracted and the result written. Twelve such tests were made, six times with a time-limit of 40 seconds, six times with a time-limit of 30 seconds. In cases where a subject completed the series in less than the allotted time her time was recorded. The key numbers were 3, 4, 5, 6, 7, 8, each added in one test, subtracted in another. Four tests were made in succession, the order in which they were given being as follows:

I.	7 added 3 subtracted	40 sec.
	4 added 5 subtracted	30 sec.
II.	5 added 7 subtracted	40 sec.
	3 added 4 subtracted	30 sec.
III.	6 added 8 subtracted	40 sec.
	6 subtracted 8 added	30 sec.

64	72	47	30
49	35	43	56
62	51	35	44
57	30	64	31
68	56	49	37
74	44	67	60
53	36	28	71
67	73	46	48
25	63	55	53
40	47	65	61
61	43	70	36
71	66	41	42
33	69	62	34
38	37	25	39
28	39	40	33
65	32	57	73
41	59	26	38
50	31	68	63
42	60	66	58
58	48	27	32
52	54	51	59
70	46	69	52
26	55	29	45
34	27	74	72
45	29	50	54

As we now know through the work of Browne,[44] Stone,[45] and others, the adding and subtracting abilities are two very different things; also some figures are easier to handle than others, a combination such as 9 + 2 being different from and easier than 2 + 9. These facts complicate the issue.

However, it seems clear that adaptation to the test does bring about a practise effect in the first few trials. The speed with + 8 in the last of the twelve tests is for every subject save Ji. greater than for + 7 in the first of the twelve.

By any rational estimate also the second day's records are above the first in general, and in the case of all but one of the subjects measured. They were so probably for Bu. also.

Using the easiest set of these additions of a 1 place to a 2 place number (+ 3), we find the time per operation to be Bu., .76 second; Gr., .96 second; Ji., 1.04 seconds; Le., 1.43 seconds, and Mo., 1.43

[44] "The Psych. of the Simpler Arithmetical Processes," *Am. J. of Psych.*, **17**, 1906.

[45] "Arithmetical Abilities . . .," *Col. Contr. to Educ.*, **19**, 1908.

TABLE VIII

RESULTS IN THE ADD AND SUBTRACT COLUMNS TEST FROM THE SHORT-TERM PRACTISE GROUP

A = amount done in time limit.
E = errors. T = seconds actually taken.

Column		1	2	3	4	5	6	7	8	9	10	11	12
Operation		+7	—3	+4	—5	+5	—7	+3	—4	+6	—8	—6	+8
Time limit in seconds		40	40	30	30	40	40	30	30	40	40	30	30
Bu.	A	?	?	?	?	25	25	25	25	25	25	25	25
	E												
	T					23	27	19	24	22	34	24	25
Gr.	A	21	?	22	20	25	24	25	17	25	25	25	25
	E	1		1						1			
	T					34		24		34	36	24	25
St.	A	13	22	11	12	25	21	25	21	25	21	20	21
	E		3		1							1	
	T					38		26		34			
Ji.	A	20	?	22	14	16	11	21	14	21	11	13	9
	E									1			
	T												
L.	A	9	21	16	11	18	17	21	16	24	20	16	20
	E									1			
	T												
Mo.	A	18	18	18	13				13	25	14	12	17
	E										1	1	
	T									38			
Ba.	A	19	12	19	15								
	E		1										
	T												
Bf.	A	17	21	25	13								
	E			1									
	T												

seconds; a median of 1.04 and an average of 1.12 seconds. On March 15 the short-term group was tested with 100 mixed examples, such as 9 + 7, 8 — 3, 6 — 2, 5 + 8, etc., 70 seconds time being given. The results were Bu., 100; Gr., 100; Ji., 69; Le., 63; Mo., 67; Ba., 64; Bf., 63. Le. made 1 and Ba. 2 errors. The median time per operation was thus 1.04 seconds, as for the easiest addition to a 2-place number. The average time was probably .9 second. In adding in columns with 5 two-place numbers, for example, in which about three fourths of the additions are to a two-place number, and in which the number added is more often harder than easier than 3, the results were, after the first trial, an average of .67 second per operation (median .87 second). Although the average especially is perhaps too low because the number of actual conscious operations was probably reduced by grouping in the case of the more rapid workers, the fact remains that the mere writing time for a two-place number may,

especially with slow writers, be greater than the time required to add a one to a two place number without writing. One has only a choice of evils. Column addition permits grouping and so mixes the rate of association with the power to associate three numbers with their sum in one connection. A test in writing additions and subtractions with two place answers measures the rate of mere writing in very rapid computers or very slow writers.

Noun and Adjective.

Two blanks with 20 adjectives on each were arranged as follows:

I

Complete the following sentences, after the model of the first one, that is, by adding to each a noun at the beginning, and a second adjective at the end—the whole to make sense:

The hill	is	high	and	wooded.
	‘‘	soft	‘‘	
	‘‘	cold	‘‘	
	‘‘	new	‘‘	
	‘‘	smooth	‘‘	
	‘‘	red	‘‘	
	‘‘	round	‘‘	
	‘‘	windy	‘‘	
	‘‘	clean	‘‘	
	‘‘	bent	‘‘	
	‘‘	wooden	‘‘	
	‘‘	deep	‘‘	
	‘‘	empty	‘‘	
	‘‘	narrow	‘‘	
	‘‘	loose	‘‘	
	‘‘	bitter	‘‘	
	‘‘	level	‘‘	
	‘‘	stale	‘‘	
	‘‘	oily	‘‘	
	‘‘	heavy	‘‘	
	‘‘	woolen	‘‘	

II

Complete the following sentences, by adding a subject and an additional adjective, as in the first sentence:

Her taste	is	refined	and	delicate.
	‘‘	portable	‘‘	
	‘‘	unexpected	‘‘	
	‘‘	ridiculous	‘‘	
	‘‘	interesting	‘‘	
	‘‘	imported	‘‘	
	‘‘	probable	‘‘	
	‘‘	tapering	‘‘	
	‘‘	dangerous	‘‘	
	‘‘	complete	‘‘	
	‘‘	unusual	‘‘	
	‘‘	metallic	‘‘	
	‘‘	spacious	‘‘	
	‘‘	painless	‘‘	
	‘‘	excessive	‘‘	
	‘‘	seasonable	‘‘	
	‘‘	desolate	‘‘	
	‘‘	frequent	‘‘	
	‘‘	distinct	‘‘	
	‘‘	select	‘‘	
	‘‘	temporary	‘‘	

A score of 1 was given for each appropriate word written, making 40 the maximum score for a test. Sometimes an indeterminate adjective such as ‘‘nice’’ or ‘‘long’’ would be written several times in succession, and the possibility of this detracts from the value of the test. One subject wrote the pronoun ‘‘it’’ instead of a noun, as directed, and so made a low scoring; otherwise this seems an easy test, for the average accuracy score was 38, or 95 per cent.

The short-term group took this test four times only, the first time with a time-limit of 120 seconds, the other three times by the amount-

limit method. The average time taken to finish was 135 seconds, A.D. 27, or an average speed per word written of 3.37 seconds. There was a slight practise effect in speed even with so few tests, but none in the accuracy. It was written more slowly than the *opposite* and *subject-predicate* tests, but this may be due to the arrangement of the blank, and the need of an additional movement of the hand. Blank I. is, so far as the records from six subjects go, much easier than Blank II., taking only about three fourths as long with equal precision.

English and Nonsense.

The following blank was used three times, a time-limit of 60 seconds being given for each section with 3 minutes interval between the sections.

A. Mark the (familiar) English words among the following groups of letters:

nop	yas	jeb	cug	pin	wam	hay	bot	hub	kib
max	dug	faw	rab	sid	ven	mar	pid	baw	moy
mud	yim	nam	lan	ram	rox	fub	hon	tey	deb
pow	was	jig	ges	lud	wid	jom	kus	dix	bag
cay	yut	dam	lax	sor	not	har	vim	pab	fon
tus	rit	kay	bir	wep	bow	lix	mur	seg	voy
sir	pex	heg	rum	gid	neg	fim	tip	loy	dut
wut	tox	gem	ruy	gor	vig	jad	kow	ton	sut
tir	hig	med	fox	bep	nis	vun	dow	gax	can
jup	nun	yow	mig	dat	tar	soy	few	lun	taw

B. Mark all groups of letters in the following list that are not (familiar) English words:

men	sar	bet	won	pox	hus	nib	ket	sum	hip
tug	mop	jaw	bux	cub	gas	pay	rib	her	num
vat	nay	gup	bun	fit	keg	sop	yes	com	fur
pum	web	ten	wox	dip	jug	sew	jis	toy	gig
lip	tar	jet	pus	rob	feg	coy	win	kid	gum
pew	mix	lep	sar	job	vap	bid	yeb	den	low
sap	ren	fow	new	red	lug	hod	kin	dot	ses
bip	led	war	his	tid	buy	sex	did	rag	hop
yew	mub	got	tax	put	hen	vot	jar	key	him
fad	tub	nor	fix	pem	vow	doy	let	nex	lay

Introspectively it was difficult to take B so soon after A, so that the blank might be cut in two instead of being used as it is. Another difficulty was found in the arrangement of the syllables. There was a tendency to work by vertical columns rather than across the sheet, and section B was confusing for the eye. Either explicit directions should be included, or the syllables printed in even columns.

No one made a perfect record in the time given, but in about all of the "Mark English words" tests, and in some of the "Mark nonsense words" tests the entire blank was gone over within the time, the rest of the time being spent in looking back for omissions. Since, moreover, there were many of both omissions and errors, the measurement of the time of the process is not feasible.

The second test is much harder. The requirement in it of equating time, errors and omissions in the case of almost every subject is troublesome. This difficulty exists to a less degree with the "Mark English words" test.

The amount of improvement due to familiarization with the plan of the test would not apparently be so great as to be very troublesome. When the same blank was used twice, as here, the change of the third over the first trial was for the *marking nonsense* words about 25 per cent. more words correctly marked, and about 30 per cent. fewer words wrongly marked, with a slight increase in omissions.

The remaining three tests were not given each sufficiently often to allow discussion of any practise effect. They were included for purposes of comparison and correlation when taking one or two trials; so that the "short-term group" becomes, to all intents and purposes, nothing more than an "instructed" group in those tests, except for their general experience of test conditions.

B. *Relative Value of these Tests*

The question of the variability and correlation of these association tests will now be taken up.

The resemblance between an individual's average ability in the *first idea, day opposite, vertical opposite, preceding letter* and *complete the word* tests combined, and his ability in each of these tests separately, was calculated in order to discover the extent to which each single test is significant of the more general ability. This resemblance was calculated both from the percentage of unlike-signed pairs, and also by the Pearson coefficient of correlation.

In the case of these and all correlations to follow, the reader will understand that I am not measuring the correlations between the true abilities which would be found from an infinite number of trials with each test, but only the correlations between the measures got from 1, 2, 3, or 4 trials, as the case may be. The question is not of the significance of certain traits in human nature, but only of certain previously defined tests of those traits.

It will be understood also that other results, mostly from only

10 and in some cases only 6 individuals, are very unreliable. They are however much more reliable than mere opinions.

The performances of the 36 individuals in the "instructed" group were thus correlated with the following results:

TABLE IX

		$\cos \pi U$	r	(Closest correlation = 1)
Average of these five tests and	First idea	.749	.623	2
	Day opposite	.844	.671	1
	Vertical opposite	.509	.615	3
	Preceding letter	.368	.484	5
	Complete the word	.425	.607	4

Thus by both methods the *easy opposites* seems to be the best test so far as it measures the element common to all these tests on association. By both methods also the *preceding letter* seems the poorest.

Next were used the results (in the first two trials) of the ten individuals in both the long-term group and the short-term group in the following tests: *first idea, vertical opposite, day opposite, preceding letter, complete the word, free association, subject-predicate, difference between, addition, Ebbinghaus combination.*

Again each test was correlated with the average for all, with the following results.

TABLE X

	$\cos \pi U$	r	(Closest = 1)
First idea	.22	.39	8
"Vertical"	.92	.48	3
"Day"	.79	.71	1
Preceding letter	.81	.42	4
Complete word	.37	.09	9
Free association	—.13	.11	10
Subject-predicate	.37	.47	6–7
Difference between	.64	.23	6–7
Ebbinghaus combination	.66	.67	2
Addition	.79	.39	5

The two methods do not agree so well this time, but again the easy list of opposites correlates high. The *preceding letter* correlates rather low by the Pearson coefficient method, high by the percentage of like-signed pairs. As this latter method takes account only of number of cases of difference whereas r is affected as well by the amounts of difference, it is obvious that a few cases of wide divergence from the average, or in other words a subject making an unusually low record in a certain test, will bring about the discrepancy between the two methods. On examining the original data this is precisely what is found: one subject usually far below the

average made a very good record at the second trial, and one of the very best subjects made the lowest record of anybody at this *preceding letter* test. The Pearson coefficient is greatly affected by these records, and is correspondingly low; by the percentage method their influence is only slightly felt.

Complete-the-word, which was low for the instructed group is also low for these two groups, extremely so by the Pearson coefficient. The other test with very low correlation, the *free association* has inverse relationship by the percentage of like-signed pairs. This means that although the majority of subjects reacted differently in this test from their average reaction in association tests, yet their individual records differ only slightly from each other—the A.D. for this test being very low.

The *Ebbinghaus Combination* test correlates fairly closely by both methods.

The *Free association* test correlates so slightly probably because, as was shown, it is largely a test of the rate of writing for many subjects.

The value of each test of association has been discussed from two standpoints thus far, that of significance measured by highest correlation with the average of all tests in the series and that of least disturbance by practise. A third standard would be that of ascertaining for each test the unreliability of any given number of trials. Where possible this has been measured in the case of: (1) the first four or five records of each member of the short-term practise group, and (2) the first five and sometimes the last five records of each member of the long-term practise group. The average results of (1) and of (2) are presented in the following table in percentage statements. The higher the figure the greater the unreliability of a single trial and vice versa. To this table is added a column to give the number of trials that would be needed to reduce the unreliability to 1 per cent., and a column to give the consequent time it would take to get such reliable information about a person's ability in that test, using as a basis for this calculation the average time taken in an amount-limit test, the time allowed in a time-limit test.

Such determinations are difficult because of the practise effect, and the difference in difficulty of different blanks of the same series. From the gross differences found in an individual's trials, one must, in order to get an approximate measure of how much difference is due to chance variations in the individual, eliminate these two added causes of difference. This can be done only approximately and by more or less arbitrary criteria.

In tests involving differences in quality as well as rate of achieve-

ment there is the further difficulty that one performance may differ from another in quality and in speed or vice versa. The reliability of the test as a whole as a measure of efficiency in the function in question can then be determined only after the combination of the measures for quality and speed into a single measure.

The method taken may be shown best by an example. The records of the three long-term subjects in the " day " opposite test were:

TABLE XI

N.		W.		F.		Av.	
Amount	Quality	Amount	Quality	Amount	Quality	Amount	Quality
13	25	11	21	12	22	12	22.6
15	29	12.5	25	13	26	13.5	26.6
15	29	13	25	13	26	13.6	26.6
17	31	14	26	14	26	15	27.6
15.5	29	14	26	14	27	14.5	27.3

Since the quality was substantially equal throughout for each individual, the reliability may be measured from the differences in the amount score alone. Since, as will be shown in a later section, individuals cluster around a central tendency in respect to changes in the rate of improvement, the general practise effect shown in the average column may be applied to each individual. That general effect smoothed may be taken as 12.5, 13.5, 14, 14.5, 15. So it may be assumed without great inaccuracy that, apart from the chance variations of the subject, the records would have been approximately—

N.	W.	F.
13.5	11.5	11.5
14.5	12.5	12.5
15	13	13
15.5	13.5	13.5
16	14	14

The deviation of the single trials due to the person's varying condition are then for

	N.	W.	F.
	.5	.5	.5
	.5	0	.5
	0	0	0
	1.5	.5	.5
	.5	0	0
A.D.	$\overline{.6}$	$\overline{.2}$	$\overline{.3}$
In per cent. of Av. Amt.	4.0	1.5	2.3

So far as these three subjects go, the probable average divergence of the result obtained from a single trial with the " day " test from the probable true result is then 2.9 per cent. of the former's amount.

To show the reliability of these estimates of reliability themselves, the results from all the short-term and from the long-term subjects are given separately.

TABLE XII

Relative Precision of Association Tests

Test	No. of Seconds for 1 Trial.	Probable Average Divergence of the Result Obtained from 1 Trial from the Probable True Result, in Per Cents. of the Former: Short Term Data	Long Term Data: Early Trials	Long Term Data: Late Trials	Combined Estimate	Approximate No. of Trials Necessary to Measure a Person with an Average Divergence of 1 Per Cent.	Approximate Time of Tests so to Measure a Person
						49	12¼ min.
Easy opposites [day, good, great, high]	30	6.9	2.9		5	25	12½ "
Hard opposites [vertical, serious]	30		7.4		7.5	56	28 "
Addition [of 5 two place numbers] ..	60	6.0	6.5	5.1	6	36	36 "
Preceding letter	15	10.0	12.4	18.1	13	169	42 "
Complete the word .	15	12.6	8.8	11.2	11	121	30 "

The facts in the case of the *subject-predicate, add and subtract columns, mark nonsense* and *English words* are too intricate to allow even an approximate estimate. So also with *difference between, Ebbinghaus combination, noun and adjective,* and *free association* starting from one given word, though these four are all apparently very much more unreliable than those listed. It appears then that for freedom from ambiguity, significance as a symptom of the condition of the association processes in general, freedom from disturbance by adaptation to the test shown in great early practise effect, and reliability, the best single written test of these is one in giving *easily thought of opposites.* In administering it, skipping should be allowed.

2. Tests on Memory

A. Descriptive

Along with these tests on association another group of tests on memory was given. Four memory tests are given to the freshmen, the *auditory figures, visual figures, logical memory* and *retrospective memory.* The method of giving them is as follows. For the *auditory figures,* each series of 8 numerals is read aloud at a rate of about 2 per second, after which the subject writes them down " in the order given." In *visual figures,* corresponding sets of 8 numerals are shown one at a time at the same rate. These numerals (Willson's black gummed) are mounted on cards, held in the hand and exposed

by turning them singly to face the subject. In *logical memory,* a passage—to be quoted later—is read to the subjects who then write as much of it as they can. Attempt is made to give the thought completely, and the words where possible. In *retrospective memory,* the subjects are asked to reproduce a line 5 cm. long which they drew as a perception-of-size test at the beginning of the hour, also to " do with it as they did before."

Other visual and auditory tests were used with the practise groups; a few other paragraphs were used though no other change made in the logical memory test; but no other " retrospective " memory test at all similar to this was devised.

The classification into " auditory, visual " and the like may well seem misleading, as it by no means implies that auditory stimuli are remembered in auditory terms, nor, more usually, that visual stimuli will not be translated by the subject into auditory terms. No warning is given to the freshmen with regard to this, and observation shows that the great majority of them do repeat orally the numerals presented visually. Any comparison of tests, then, does not signify a comparison of kinds of memory, but of varied stimuli or material, and varied ways of presenting material. On the report sheet sent to the freshmen care is taken to say " numerals heard," and " numerals seen "; but here, for brevity's sake, the more usual designation of auditory, visual, etc., will be adhered to, with the understanding that the words refer to stimuli, not to memory terms. For convenience sake also, the tests with auditory stimuli are discussed first, those with visual stimuli later, though the related words might possibly be classified as a logical memory test.

Auditory Figures.—Experience with this familiar test as given to the freshmen shows that most of them group the 8 numerals in two groups of four. Enquiry reveals that many depend upon a memory after-image for the last four, and memorize the first group only. The average number correctly remembered is 7.6 for the men, 6.7 for the women. This test is thus too easy, many of the individuals obtaining perfect scores.

The chief difficulty in comparing people's work on memory lies in the variable methods of scoring, especially with regard to transpositions. If the order is 76431528, and a subject writes 7463 . . ., some experimenters call it two errors because both the 4 and the 6 are in the wrong places; other experimenters call it one error because by making one change—by " lifting " the 6 over the 4, it is corrected. The latter method seems preferable. Supposing a subject were to write 87643152, eight errors would be scored by the first method since each numeral is misplaced; by the latter method only

one error is scored, since one change would set all right. Also, a misplacement error would be rated more nearly as an omission. A subject writing 76–31528 would be scored one error for omitting the 4, but two if he places it before the 6, by the first method; in either case he is scored just one error by the latter method, putting misplacements and omissions on an equal basis.

In the work to be reported on therefore, the second method was used, only that a positive score was used instead of counting the errors. Thus each numeral given correctly was scored 1/2, and if it was in the right place—interpreting this as relative place not absolute place—it was scored 1/2 more. This modification has the advantage of being rapid to use in determining the score, especially of the different kinds of material used in the tests. It is also much easier and can be used more rapidly than the Spearman " foot-rule " method, or the modification recommended by Whipple (" Manual," p. 266). If it is too cumbersome when it comes to calculating correlations, the figures can be very quickly read off as numbers of errors.

According to this method the average freshmen scores would be, as before, 7.6 for the men, 6.7 for the women.

To the " instructed " group of eighteen subjects, two sets of ten numerals were given, with an average score of 7.2 figures remembered for the men, A.D. .75; and 6.1 for the women, A.D. .85. This agrees with the superiority shown by the men over the women in the freshmen results, though showing lower scores.

The short-term group made six trials with ten numerals at a time, with an average score of 8.8 numerals remembered, A.D. .7. The series of 10 was long enough to measure all in this group. No practise effect was observable.

The long-term group made twenty trials with ten numerals at a time. One subject made only four errors in the whole series, her memory span for this being evidently greater than ten; in consequence her records were not used in estimating practise. For the other two subjects the average score was 9.55, the first day's average deviating by —.55, the last by —.5, or taking the first two and the last two trials, the deviation at first was —.45, and at last +.2.

For these two subjects also the list of 10 was not long enough to measure the practise effect accurately, there being numerous perfect scores. Their records were, in order (in errors):

N.	1	2	1	0	1	2	1	0	0	1	0	0	1	1	0	1	1	0	0	2
F.	3	1	1	2	3	0	1	0	0	2	1	0	0	1	0	2	2	0	0	0

Two other auditory tests were used, (1) series of fifteen *related words,* and (2) *mixed* series of unrelated units, including besides

LISTS OF RELATED WORDS

I	II	III	IV
College	See	Book	Holiday
course	sensation	author	excursion
grade	perception	style	boat
graduate	interpret	classic	train
senior	illusion	literature	ticket
dues	cortex	essay	early
money	hemisphere	poem	seat
purse	ganglion	rhyme	hot
lost	dendrite	meter	window
advertise	branch	scan	draught
reward	conduct	quantity	cold
deceive	intercept	Latin	bronchitis
angry	numb	translate	doctor
threaten	injury	language	medicine
blows	paralyze	accent	cure

V	VI	VII	VIII
Noise	Sunset	Time	Black
cat	dusk	test	negro
baby	lamp	write	Africa
child	table	quickly	Congo
kindergarten	play	maze	Leopold
child-study	deal	difference	rubber
psychology	lead	sorting	cruel
Thorndike	queen	color	atrocity
chickens	trump	forms	remonstrate
monkeys	short	remember	America
bananas	partner	auditory	Rockefeller
fruit	trick	score	millions
skin	point	improve	oil
slice	rubber	average	monopoly
supper	stop	twenty	trusts

IX	X	XI	XII
Picture	Child	Sunday	Finance
photograph	teacher	rest	stocks
pose	rude	church	rise
recognize	naughty	sing	fortune
because	punish	choir	invest
older	sorry	organist	dividends
friend	forgive	training	railroad
together	better	abroad	anthracite
travel	promise	Germany	Phoebe
foreign	broken	Berlin	advertisement
steamer	hardened	university	magazine
seasick	discourage	philosophy	story
improve	report	research	read
turbine	trouble	valuable	hammock
Cunard	consult	publish	trees

XIII	XIV	XV	XVI
Dog	Sky	Paper	Teach
kind	cloud	envelope	physics
terrier	raining	write	experiment
rats	wet	letter	light
hunt	spoilt	parents	refraction
catch	new	away	angle
trap	expensive	seaside	measure
poison	money	sands	survey
antidote	draw	bathe	instrument
doctor	bank	swim	careful
ambulance	cashier	deep	understand
policeman	dishonest	cramp	accurate
Irish	abscond	drowning	rely
Murphy	scandal	revive	promote
milk	newspaper	thankful	successful

words, numerals, letters of the alphabet and sounds such as clapping the hands, tapping, ringing a bell, shuffling the feet, whistling, etc., the necessary movements being out of sight of the subjects.

The short-term group made five trials using series I., II., III., IV., and VI. Besides scoring in the manner described, note was kept of whether the errors were those of omission or misplacement, or whether extra words were put in. At first sight it would seem best to handle this score by keeping it in terms of errors made; but as the score is given for the right words in the right order, additional words practically counted as errors. From the point of view of interest in individual differences, however, it was felt worth while to keep track of the number and occasion of additional words; also to note whether any one list seemed more tempting to the imagination than others. In a total of 30 records, eight of them had extra words, one subject supplying them three times. She remembered the greatest number of words correctly. The subject with the lowest score put in extra words twice. Every subject misplaced some words, the one with the best score doing so most often. The average score was 8.9 words A.D. 2.5. There was no practise effect discernible.

The long-term group in a total of twenty trials made an average score of 12.6 words, A.D. 1.5. The first two trials deviated by — .75, the last two by — .15, but there seemed no certainty of practise effect. The lists of 15 words were just long enough to measure the most capable of these subjects; toward the end of practise a list of 16 would be better for regular use. No particular list seemed specially liable to error. The subject with the highest and least variable record wrote the fewest extra words, and made six perfect records.

Both of the other subjects showed considerable variation, one having five perfect records, but misplacements 50 per cent. of the time, the other having no perfect records, and only three free from extra words or misplacements. The one with the greatest number of misplacements also wrote the greatest number of extra words. The subject who had so good a record with the auditory numerals was not the best in this test.

Auditory Mixed.—The object in giving this test was to present material absolutely disconnected, yet with each of the units in the list having its own meaning. Even with nonsense syllables some fanciful connections are usually made, so that it was not supposed that artificial associations could be entirely avoided; nevertheless by introspection there seemed to be very few of them in this case. There is some difficulty in presenting nonsense syllables orally, but with this incongruous yet senseful material there is less danger of errors in hearing on the part of the subjects. The tendency to groupings of four was broken up somewhat by the introduction of the various sounds or noises (shown in the list by italics). By introspection this test proved difficult and irritating to those accustomed to the other material.

The lists used were as follows:

(1)	(2)	(3)
Carriage	Distance	Oo
F	as	but
adversary	*whistle*	16
preach	flag	resting
stamp with foot	require	*clucking noise*
lamp	38	organ
never	other	3
ring a bell	harper	spring
K	*clap hands*	W
green	H	matches

(4)	(5)	(6)
And	Monstrous	99
20	(*jingle keys*)	monotone
ring a bell	X	*scrape with foot*
wall paper	Symphony	alphabet
stampede	*tap with pencil*	tomahawk
tap with finger	she	*jingle keys*
M	*whistle*	asleep
symmetry	bugle	purple
stamp with foot	typewriter	*tap, or clap*
56	ice-cream	because

The short-term group made only 2 trials, with an average score

of 8.15, A.D. .45. The long-term group made 20 trials, with an average score of 9.2 of the ten remembered, A.D. .35. The detailed results were, in order (in terms of errors)—

N.	3	5	2	1	1	2	2	1	2	3	2	1	1	2	1	2	1	1	0	2
W.	0	2	2	2	2	1	1	1	2	1	3	1	1	1	0	3	1	1	1	2
F.	2	2	2	2	3	1	1	0	3	1	2	1	2	0	1	3	2	1	1	2

There was no practise effect discoverable. The subject who was so very competent with the auditory figures was also the best in this test. The misplacements were unfortunately not noted, so that no comparison can be made in this respect with the *related words.*

Visual Figures

Three sets of eight numerals are shown serially to the freshmen. No apparatus is used, and some little practise is required on the part of the experimenter to expose the cards regularly and at a convenient angle. As said before, no warning is given about not repeating to one's self orally what is shown. The men remember 6.9 correctly on the average, the women 5.7.

Two of these sets were used with the "instructed" group. The men made an average score of 5.85, the women of 5.15, again agreeing with the freshmen results in the superiority of the men's record over the women's, though showing lower scoring for both men and women than in the case of the freshmen. The percentages would be 73 and 64.

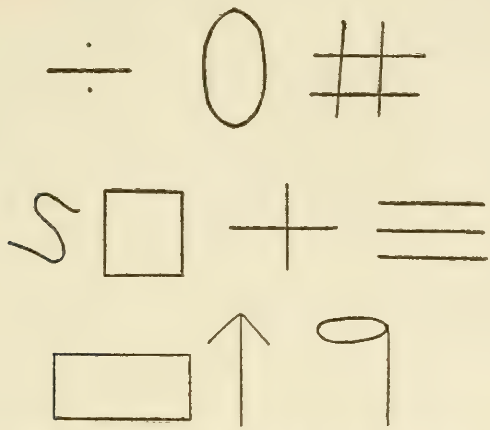
The short-term group made 5 trials with sets of 8 numerals; their average score was 7.5, A.D. 0.5. Series of 8 are thus too short for an adequate measure of visual as well as auditory memory.

The long-term group made 20 trials with sets of 10 numerals. For the first four trials cards were used as for the freshmen. After this, as a screen with a slit was in use for other visual material it was used for the numerals also. This screen was a very simple affair of pasteboard with a 2-inch square opening in the middle. The visual stimuli were written or drawn with charcoal on a long strip of cardboard which was pushed along behind the screen, allowing one second for the exposure of each unit in the series. By reversing the strip, one series could be used as two different tests on different days. Sixteen trials were made with this, making twenty in all. Even series of 10 numerals are too short for adequate measurement of these subjects, perfect records being made frequently after the first three trials.

Their average score was 9.4, A.D. .5, the range from 8 to 10. The first day's average deviated by — .1, the last by + .8.

Other visual tests were: *grouped forms, serial forms, grouped objects, serial objects, forms recognized.*

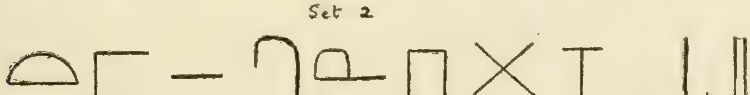
Grouped Forms.—Five different sets were used, one of which was as follows:



These forms were drawn roughly with crayon on a small blackboard which could be turned and exposed to view for 10 seconds, then turned away again. The short-term group made only two trials with sets 2 and 4. Their average score was 5.4 forms, A.D. .9. The long-term group made 10 trials, average score 8.15 forms, A.D. .1.0. The first day's trial deviated by — 1.35, the last by + .35. It had been intended to make 20 trials with this as with the others; but very soon the question arose whether it was not much easier to look at a group of 10 for 10 seconds than to see 10 units one at a time for one second each, in the same way that the numerals are shown, with no chance of looking twice at any one of them. It was decided to compare the grouped with the serial method, both for forms and objects, though cutting down the number of trials to 10 each, for this group of subjects.

Serial Forms.—The cardboard screen and strip, as described before, were used in this test. The sets of forms were similar to those used in the *grouped forms* test. Two of them are here reproduced.

Set 2



Set 4

The short-term group made 4 trials, of which the average score was 6.5, A.D. .75. The averages of the successive trials were 5.66, 5.83, 7.07, 7.43 showing a greater gain for them in this test than in the other immediate memory tests. Probably this is due to the initial comparative unfamiliarity of the material used.

The long-term group made 11 trials, average score 7.95, A.D. .95. The first day's average deviated by — 1.95, the last by + 1.7, showing a very great practise effect.

Grouped Objects

Ten familiar objects chosen from about 25 in daily use, such as a watch, box of matches, bunch of keys, spool, envelope, pack of cards, books, scissors, fish-hook, soap, were arranged in the same groupings as that used for the grouped forms, a row of three, a row of four, a row of three, thus,—

X X X
X X X X
X X X

on a small table behind a screen. At the signal the screen was raised for 10 seconds. The subjects then wrote down the names of the things seen, grouping the names as the objects had been grouped.

Only the long-term group practised with this test, their average score in ten trials being 8.85. The first day's trial deviated by — 1.25, the last by — .1. On the fifth and eighth trials, perfect scores were made, however, by all three subjects.

Serial Objects

In this test, the same sort of objects were picked up one at a time and shown for one second each above the screen.

The long-term group in ten trials made an average score of 9.3, the first day's average deviating by — .3, the last by + .1.

So far then as serial grouped method is concerned there seems, by examination of the accompanying table,

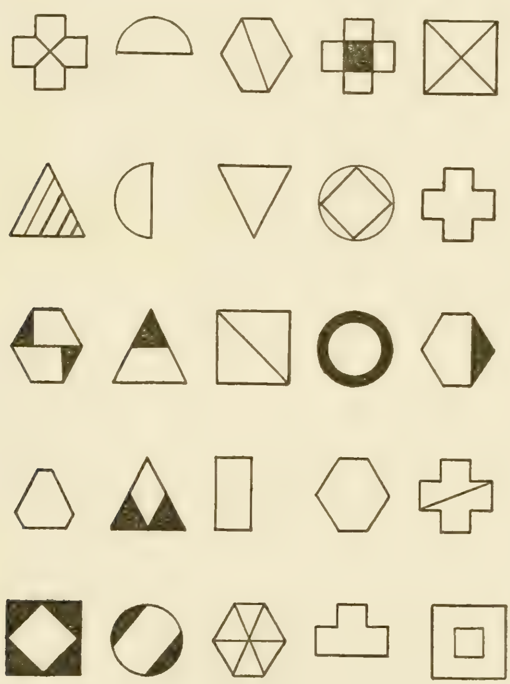
	Serial	Grouped	
Short-term	6.85 (4 trials)	5.4 (2 trials)	Forms
	6.0 (first 2 trials)		
Long-term	7.95	8.15	
	8.3	8.75	Objects

to be a slight balance in favor of the serial method, probably because this is the familiar method used for numerals, and in auditory stimuli. Introspectively, the long-term group found the *grouped*

forms easier than the *serial forms.* The reason is, perhaps, that with the latter method the second of exposure is not always sufficient for the recognition of some of the forms, whereas when grouped, the total 10 seconds can be distributed in the most economical manner, the eyes pausing longer, or returning to those forms not so readily apperceived. In the case of objects shown, this factor of apperception scarcely entered in, as each object was readily recognized, and mentally named in its one-second exposure. A slightly higher score was made on the average for objects shown serially than shown grouped.

Forms Recognized

The blanks used in this test are reproduced on this and the three following pages.



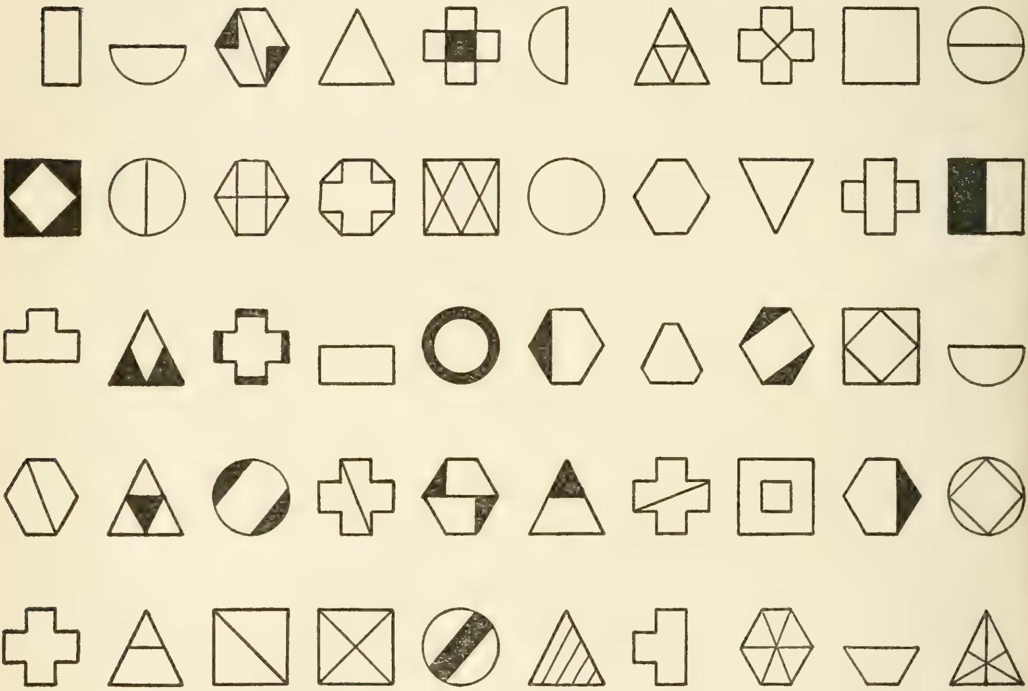
1 (*a*)

The subject is given the small sheet with instructions to study it in any way preferred till at the end of 60 seconds he is given another sheet on which he is to mark as quickly as possible all the forms he remembers having seen on the first sheet. It will be noticed that on (1) 24 can be marked, on (2) only 18.

The time taken to mark the second sheet is noted, also the total number marked, and the number correctly marked.

Set (1) was given to the Barnard freshmen of the class of 1912. The average time taken by 49 of them was 66 seconds, A.D. 16.2, with 15.6 correctly marked, A.D. 2.3, and 5 wrongly marked.

Six members of the short-term group and the most rapid worker in the long-term group made one trial with this set. Their average time was 81 seconds, or, not counting N., 88 seconds, A.D. 22.5 with 15 correctly marked and 2 wrongly marked. These subjects made trial



1 (*b*)

also with (2), where their average time was 115 seconds, A.D. 33, with 9.5 correctly marked, A.D. 1.3, and 3.5 wrongly marked. It is much more difficult than set (1).

The attempt thus to measure memory by a combination of the amount recalled, the quickness with which it is recalled, and the errors made, should be carried on with better material. The results obtained here are of value only for measurements of the significance of this particular test by its correlations.

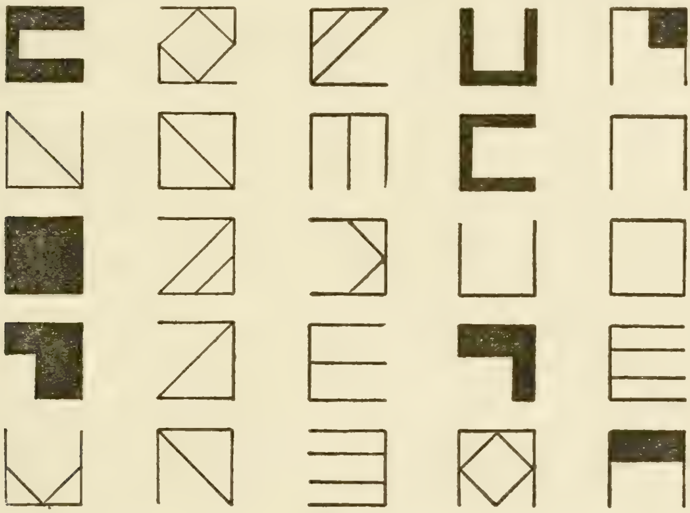
Two other memory tests were given, the *logical memory* and the *retrospective memory*.

Logical Memory. A paragraph is read aloud to the subjects who then write out as much as they remember of it, stress being laid upon

the matter rather than on the words remembered. To the freshmen the following paragraph (I.) is read:

I

Tests such as we are now making are of value both for the advancement of science and for the information of the student who is being tested. It is of importance for science to learn how people differ and on what factors these differences depend. If we can disentangle the complex influences of heredity and environment we may be able to apply our knowledge to guide human development. Then it is well for each of us to know in what way he differs from others. We may thus in some cases correct defects and develop aptitudes which we might otherwise neglect.



2 (*a*)

The men remember 44.5 per cent. of the ideas contained in it on the average, the women 51.2 per cent.

The short-term group made four trials, once with this paragraph, and once with each of three others II., III., and IV.

Other Passages Used

II

Could the young but realize how soon they will become mere walking bundles of habits, they would give more heed to their conduct while in the plastic state. We are spinning our own fates, good or evil, and never to be undone. Every smallest stroke of virtue or of vice leaves its never so little scar.

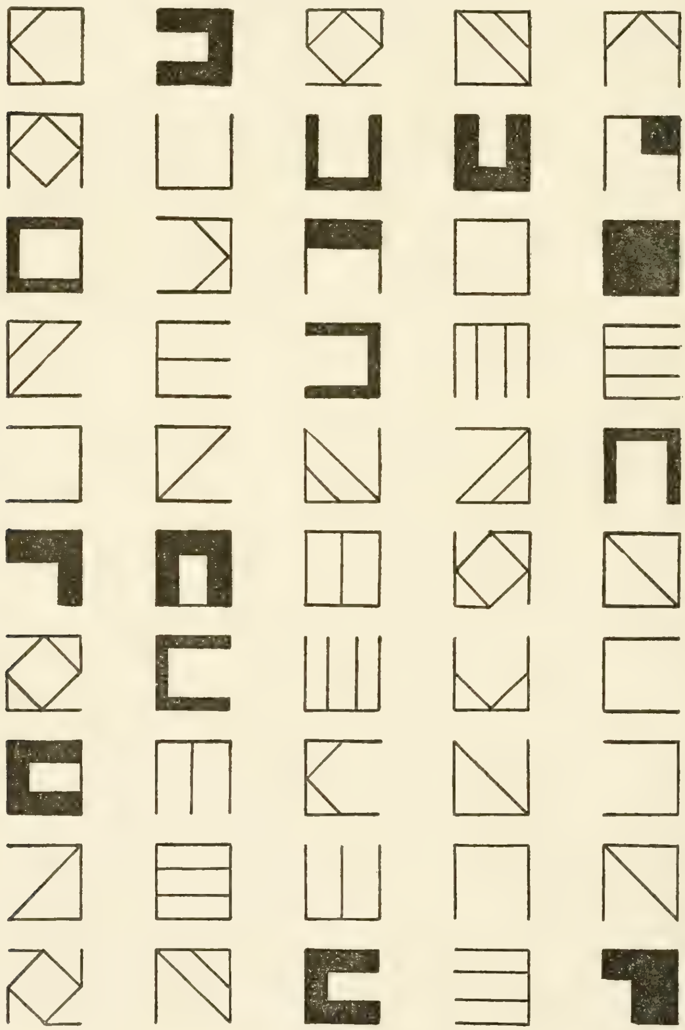
III

Measures of the variability of the individual measures are of two sorts: measures of the averaging type and measures of the percentile type.

The mean square deviation equals the square root of the average of the squares of the deviations of the individual measures from their average, median, or mode.

IV

The abstract scheme of successive predications, extended indefinitely, with all the possibilities of substitution which it involves, is thus an immutable system of truth which flows from the very structure and form of our thinking. If any real terms ever do fit into such a scheme they will obey its laws.



2 (*b*)

The average percentage remembered was 39.1; for paragraph I. alone it was 49 per cent., slightly lower than was the case with the

Barnard freshmen. These tests were given primarily as a means of estimating the significance of so called "logical memory," and no data on the effect of practise were secured.

TABLE XIII

INDIVIDUAL CREDITS FOR MEMORY PASSAGES

Graded on a scale of ten

	I	II	III	IV
Bu.	7.0	7.0	5.5	6.0
Gr.	3.5	5.5	2.0	3.0
St.	3.0			
J.		1.0	3.5	1.0
L.	5.0	6.5	1.5	3.0
M.	6.0	5.5	1.5	3.5
Ba.		6.0	1.5	3.5

Retrospective Memory.—Instead of the test given the freshmen, which consists of reproducing a line the same length as one seen and reproduced an hour previously, the long-term group made ten trials in eight of which they were asked to reproduce the list of 15 related words given as an auditory test on the previous day. On that occasion the list had of course been read, written more or less correctly and then re-read for the subjects' satisfaction in their performance, so that there had been three repetitions of the list, two of them correctly, followed by an interval of about twenty-four hours. At the third and seventh trials other material was used. Once they were asked to reproduce a paragraph used the day before in a "complete the paragraph test," and once to give the ten kinds of objects used in a "naming 100 objects" test—yet to be described. It would be interesting to prolong and vary this test indefinitely, as individuals differ so much in their ability to recall different kinds of things after different intervals, and so many human interests depend upon the accuracy and length of retention; but as the object here was merely to discover any tendency to practise effect in such mature subjects, and as time and opportunity were lacking for more prolonged series, only these ten trials were made. The score was 9.9 on the average, with no practise effect discernible.

B. Relative Value of these Tests on Memory

On the whole, there is no evidence that in any of these tests of immediate memory, a first trial measures a markedly different process from later trials after the subject is adapted to the form of the test. No great difference can exist, or it would show itself in the work of the short-term group. With the possible exception of *serial forms,*

there is no test in which the second trial shows any greater proportionate improvement over the first than the fourth or fifth shows over the third or fourth. Indeed, in almost every case *it* is among the records of the long-term group that evidence of the existence of any practise effect must be sought.

The tests rank in respect to susceptibility to practise as follows:

Very slight, not discernible in these cases	*Auditory mixed.*
	Serial objects.
	Retrospective.
Slight (less than 10 per cent. in 20 trials)	*Auditory figures.*
	Auditory words.
	Visual figures.
Considerable	*Grouped objects.*
	Grouped forms.
Most	*Serial forms.*

Certain correlations of these various tests on memory have been computed.

First of all, taking the short-term and long-term groups together, the average of the first three records of each subject in the following tests were compared, each test with the average for all six tests: *auditory figures, related words, auditory mixed, visual figures, grouped forms, serial forms.* In calculating this set of correlations the deviations of each subject in the short-term group from the average of her own group were taken, not from the average for the ten subjects treated as one group.

Next, the records of the "instructed group" with *auditory figures* and *visual figures*—18 cases, two trials for each—were correlated; also the same tests for the short- and long-term group, as above. Similarly nine subjects' records with *auditory figures* and *related words,* and five subjects' records with *related words* and *logical memory.*

Third, all auditory tests, viz., *auditory figures, related words,* and *mixed series,* were averaged, and each test correlated with the average of all, using again the average of the first three records of both short- and long-term groups.

Fourth, using 10 subjects as above, the correlation of *grouped* and *serial forms* was computed.

Last, *visual figures* was compared with *forms recognized* using the records of the 49 freshmen, and also those of the short-term group. The latter test was also compared with *grouped forms,* a supposedly similar test.

All these results are presented in the following table, where in addition to the Pearson coefficient, the rougher correlation by the

method of unlike-signed pairs is given wherever justified by the number of cases available.

It will be understood that these correlations are to measure the significance of three (or two, as noted) trials of a given test, not the true relation between an individual's total ability in one trait and his ability in another. The reader is again reminded that the results commonly from only ten subjects are only very coarse approximations, but are nevertheless by so much better than nothing.

TABLE XIV

		$\cos \pi U$	r	No. of Cases
1. Average of these six tests and	Auditory figures	.31	.51	10
	Related words	.93	.64	10
	Mixed series	.31	.05	10
	Visual figures	?	?	10
	Grouped forms	.95	.91	10
	Serial forms	.31	.45	10
2. Auditory figures and	Visual figures	0	.21	18
Auditory figures and	Visual figures	0	.17	10
Auditory figures and	Related words		.12	9
Logical memory and	Related words		.55	5
3. Average of these three tests and	Auditory figures	.48	.69	10
	Related words	.93	.58	9
	Mixed series	.93	.64	10
4. Grouped forms and Serial forms		.81	.76	10
5. Forms recognized, and Visual figures		.03	.37	49
Forms recognized, and Visual figures			—.13	6
Forms recognized, and Grouped forms			.26	6

In the first set of correlations, with varied material and including auditory and visual tests it would be surprising to find high correlations. *Grouped forms* stands out conspicuously therefore as a typical test—in so far as it measures whatever element may be common to all these six tests. *Related words* comes next by both methods of correlation, while *visual figures* is actually an inverse relationship.

In the second set it is seen that *auditory* and *visual figures* have a very low correlation, none by the percentage of unlike-signed pairs. Clark Wissler, who differentiates between numerals correctly given and those correctly placed, found correlations of .29 and .39 respectively.

The correlation of *auditory figures* and *related words* is, however, still lower, though too much can not be argued from the records of only 9 subjects. The very few records for *related words* and *logical memory* similarly cautious against too great emphasis on the higher correlation found there, though this is certainly more what might be

expected. The unreliability of these two Pearson coefficients is (P.E. *r* true—*r* obtained) .021 and .184 respectively.

In the third set, it is interesting to see that all the correlations of the auditory group are fairly high, and that *auditory figures* come out better than *related words* reckoning the Pearson coefficient only, though in the first set this was not the case. Even the *mixed series* correlates well with the average of the group, and the coefficient is higher than that of *logical memory* and *related words* (in the second set), rather unexpectedly.

Summing up this work on memory from the point of view of intercorrelations, *auditory figures* and *related words* seem tests fairly typical of any presented to the ear. *Grouped forms* seems distinctly typical as, taken all through, its correlations are high.

As to the question of the relative precision of the different tests of memory, making a reasonable allowance for practise effect, where such exists, the unreliability of single trials with the tests described are as shown in Table XV. The unreliability of a test with visual figures can not be properly estimated. The series of eight were, as has been stated, too short, and the series of ten was for the long-term group too short toward the end of practise. From the early trials of these three subjects the average divergence of the result from a single trial from the true result may be estimated as from 5 to 7 per cent. according to how the probable course of practise is estimated.

TABLE XV

RELATIVE PRECISION OF MEMORY TESTS

Test	Most Probable Average Divergence of the Result Obtained from 1 Trial from the Probable True Result, in per cents. of the Former: Short Term Data	Long Term Data: Early Trials	Long Term Data: Late Trials	Combined Records	Approximate No. of Trials Necessary to Measure a Person with an Average Divergence of 1 per cent.
Auditory words	18.0	13.1	12.8	14.6	213
Auditory mixed		4.3	3.5	3.9	15
Visual grouped forms		14.6	12.1	13.3	177
Visual serial forms	9.7	9.9	13.6	11.1	123
Visual grouped objects		5.4	10.8	8.1	65
Visual serial objects		3.1	4.0	3.5	12
Visual figures		6.7			(45)

So far as the data go, *Auditory mixed series, Visual serial objects* and probably *Visual figures* (with a long enough series) have decided advantages from the point of view of precision over the other tests. *Auditory figures* was, as given, too easy a test to measure the subjects and therefore could not be included in this list.

If a choice of tests were to be made therefore, a good test, correlating with other auditory tests and not much subject to practise with mature subjects, and requiring few trials for a fair degree of precision is *Auditory figures*. *Related words* is good except for the lack of precision accentuated by the fact that any selected list of words with its varied appeal to different types of subjects would be less simple than numerals with their greater similarity of associations.

In spite of its susceptibility to practise and the greater number of trials required to give a fair degree of precision *Grouped forms* is suggested as the best visual test for three reasons: (1) it is significant of memory in general; (2) subjects have slight tendency to repeat the name of the form, so that it appeals merely to the eye better than do numerals or objects; (3) it is equally easy if not easier to give than *Visual figures,* requiring less dexterity in manipulation. Standard groups could easily be drawn or printed on cardboard, say two feet six inches square, and thus used for small groups as well as for individual work.

These tests complement one the other and would together make an easily given, easily scored and fairly significant and precise test.

3. Tests on Perception

A. *Descriptive*

The A Test.—The following blank, here reduced in size, is used with the freshmen.

OYKFIUDBHTAGDAACDIXAMRPAGQZTAACVAOWLYX
WABBTHJJANEEFAAMEAACBSVSKALLPHANRNPKAZF
YRQAQEAXJUDFOIMWZSAUCGVAOABMAYDYAAZJDAL
JACINEVBGAOFHARPVEJCTQZAPJLEIQWNAHRBUIAS
SNZMWAAAWHACAXHXQAXTDPUTYGSKGRKVLGKIM
FUOFAAKYFGTMBLYZIJAAVAUAACXDTVDACJSIUFMO
TXWAMQEAKHAOPXZWCAIRBRZNSOQAQLMDGUSGB
AKNAAPLPAAAHYOAEKLNVFARJAEHNPWIBAYAQRK
UPDSHAAQGGHTAMZAQGMTPNURQNXIJEOWYCREJD
UOLJCCAKSZAUAFERFAWAFZAWXBAAAVHAMBATAD
KVSTVNAPLILAOXYSJUOVYIVPAAPSDNLKRQAAOJLE
GAAQYEMPAZNTIBXGAIMRUSAWZAZWXAMXBDXAJZ
ECNABAHGDVSVFTCLAYKUKCWAFRWHTQYAFAAAOH

There are 100 A's on it, and the directions are to mark as quickly as possible all the A's. Since several A's occur together more than once it might be better to tell them to mark each A.

The men take 100 seconds on the average, the women 87.3 seconds, agreeing with the general conclusion that women are quicker with this sort of test—noticing details—than are men. The general ex-

perience is that all the A's are not marked by either the men or the women, so that when using these figures comparatively, *i. e.,* when 60 A's are scored in 60 seconds for the men, and 68.7 for the women, it must be understood that they are only approximately correct, are in fact a little too high.

In testing this test, the following blanks were used. No. 2 has also 100 A's; No. 3, 50 of each of the letters A, B, K, S.

Set No. 2

GAAQYEMPAZNTIBXGAIMRUSAWZAZWXAMXBDXAJZ
ECNABAHGDVSVFTCLAYKUKCWAFRWHTQYAFAAAOH
UOLJCCAKSZAUAFERFAWAFZAWXBAAAVHAMBATAD
KVSTVNAPLILAOXYSJUOVYIVPAAPSDNLKRQAAOJLE
AKNAAPLPAAAHYOAEKLNVFARJAEHNPWIBAYAQRK
UPDSHAAQGGHTAMZAQGMTPNURQNXIJEOWYCREJD
TXWAMQEAKHAOPXZWCAIRBRZNSOQAQLMDGUSGB
FUOFAAKYFGTMBLYZIJAAVAUAACXDTVDACJSIUFMO
SNZMWAAAWHACAXHXQAXTDPUTYGSKGRKVLGKIM
JACINEVBGAOFHARPVEJCTQZAPJLEIQWNAHRBUIAS
YRQAQEAXJUDFOIMWZSAUCGVAOABMAYDYAAZJDAL
OYKFIUDBHTAGDAACDIXAMRPAGQZTAACVAOWLYX
WABBTHJJANEEFAAMEAACBSVSKALLPHANRNPKAZF

No. 3

GWBTBVKIKSCSAUEBCIWVABZSMDUBKLWHKHYCGYGK
NANNCBVBSAKOIUPEKCXVGSTVRIWYBYGKHAZLPBYO
XAPYEXXHUFSBVDYDIAZLRSATZAZVFCOFSAIPTDOK
BBISKAKHXDYIUZRHVRZYSCIGECPOFKBICBMGFSDC
YHSRMVBLYICKZBMXFVBBIKUCBZLOGLVKGFMOATUN
SHOFHXIMKUXLDZKMRYRLVUWWKYEUVECSOUWBADEX
ALUAKRMSFTGXWLVGAOWBTPODXBNSFSFSWSDRSMPO
KBRIGAXZBZACKFBBEVWCGSWBMFEMXXOKRDIWGGBL
BTPNSKBACVTCSSRKUBURUDMZEWIZFESTMZEBWAFI
BKSGYHSLSFABTLTIUDXGAKROZYKOBHEAALPMLLKC
GVCWKKPTUYUGSTSSDWNKSIEICSNBTVADKANTKKPB
UXGTSOSUZPNBKRBAFDYFOVYBMPSOMBUOPMEGKKTA
COWVFXATSVAPAKYVAHNFXSBDAZYDCFDPPKNPHAMM
XUNKDXSRAAMDVOPECXRKTLHAXVKSHYWEWMMNNHBR
SLSOZFBZGRRIIHKRLEKHEZRGSCYKUIPSLECKYNDA
UGKLLEMAXFYERKWZYSNTTUAVSNAAMNWSAODFWAEH
WBNSPAKBBAOAHPHBHNRDELDLMPWZTAIORTSKLBAZ
HNBKXPSNXAZHNIPHFGTE

The disturbing effect of adaptation and practise with this test is very slight. The short-term group using blank 2 required .783 second per A marked in their first trial of 45 seconds and .869 second per A marked in a second trial of 60 seconds. The long-term group using blank 1 required .643 second per A marked in the first, and .636 second per A in a second trial, each of 60 seconds.

An "instructed" group of eleven subjects who marked A, B and K in order in three successive trials with blank 3, took only nine tenths as long per K as per A; but the same proportionate time was taken when K was given, as the first to be marked, to one group of 18 and A to another group. The difference was therefore probably largely due to the greater ease of marking K.

To determine the relative difficulty of finding A, K, B and S on No. 3 blank, four similar groups of 19 subjects were tested, each group marking a different letter. A time limit of 105 seconds ($1\frac{3}{4}$ minutes) was allowed to mark the 50 letters. The results were as follows:

TABLE XVI

Blank	Letter	Time	Av. Marked	A.D.	No. of Cases
No. 3	A	105	41.3	5.1	19
No. 3	B	105	40.0	5.2	19
No. 3	K	105	37.5	3.9	19
No. 3	S	105	44.6	5.1	19

The time was possibly too long to measure all adequately in the case of the letter S.

The short-term group gave the following results which, in view of the probability that practise effect is very slight, may be used to estimate the relative difficulty.

TABLE XVII

Letter	Method	Time in Sec.	Av. Marked	A.D.	Sec. per Letter
S	Time limit	40	26.0	8.0	1.54
S	Time limit	30	18.0	5.0	1.67
	(Three other trials intervening)				
B	Amount limit	Av. 117	47	17 sec.	2.49
K	Amount limit	Av. 112	43.5	12.3 sec.	2.61
A	Time limit (not reached)	90	50	?	1.80
A	Time limit	60	31	5.3	1.94

K is a little harder than B as before, and S is easier than A by about the same proportion as before. A and S can not properly be compared with B and K since the announcement of a time-limit seems to have a stimulating effect.

An "instructed" group of eleven subjects in a 60 second test with the order ABKS gave averages marked of 30.1, 32.7, 27.0, and 37.1 respectively, or 2.0, 1.83, 2.22, and 1.62 seconds per letter marked. These figures where the practise effect for A in comparison with S is reversed confirm the others.

Concerning the influence of the time-limit versus amount-limit method the following records show that the former does seem to act

as a suggestion to greater efficiency. Those subjects who with amount-limit required more than 105 seconds, often completed the blank with that time limit, making as high scores for accuracy as with the longer time. The facts are:

TABLE XVIII

	Time Limit 105 Fifth Test Letters Marked	Amount Limit Eighth Test Time	Letters marked	Time Limit 105 Thirteenth Test Letters Marked	
Gr.	40	149	47	48	Marking B
L.	45	125	46	49	
M.	47	117	46	50	
Ba.	48	127	47	47	
	Sixth Test	Ninth Test		Fourteenth Test	
Gr.	39	111	41	41	Marking K
J.	49	110	50	46	
M.	42	134	45	41	
Ba.	37	127	38	37	

a — t Test.—The blank is as follows: parts A and B are generally used for separate tests.

(A)

A.

Dire tengo antipatia senores; esto seria necedad, porque hombre vale siempre tanto como otro hombre. Todas clases hombres merito; resumidas cuentas, sulpa suya vizxonde; pero dire sobrina puede contar dote viente cinco duros menos, tengo apartado; pardiez tamado trabajo atesorar-los para enriquecer estrano. Vizconde rico. Mios, quiero ganado sudor frente salga familia; suyo, pertenence, tendran. Conozco marido pueda convenirle Isabel; Carlos, sobrino. Donde muchacho honrado, mejor indole, juicioso, valiente? Quieres sobrino. Esposo parece natural, pero. Pero, pero, diablos, objeciones hacer. Posible quedandonow solos siempre hacer oposicion. Solo delante hentes eres ministerial. Pues, sidens siempre plan, dicho antes, porque hace tiempo notade cose aflige cierto. Sabes cuante quiero Carlos; consuelo apoyo; despues persona quiero mundo. Como eres buene amable, quieres porque, darme gusto, pero quisiera. Palabra cuesta trabajo; parece sino teines miedo agasajarle, manifestarle carino. Veces tratas cumplimiento veces senor. Probare; ejemplo pudiendo abandonar case negocios, deseaba hubiese acompanado viaje; preferiste sola sobrina doncella. Quise contradecir, pero para sentimento, para tambien. Voto gasta palabra, dice frases, dice; pero alla adentros quiere. Mientras estado malo, puesto dirigir casa; pardiez aunque carrera, hacia mejor; cabo tiene sobre ventaja poca edad, activadad zelo, pues para contigo digo. Siempre ordenes; dejaria matar alcanzarte billete para opera para baile. Necsitamos para felices; algo estrano, desconocido. Esta resuelto; supuesto hemos hablado esto, mismo, preciso empieces darle conocer nuestros planes. Quien mejor. Opone nunca deseos, sera facil nadie persuadirle. Probare menos, preciso sino creere tienes interes decidido proteger vizconde. Pudieras creer siempre inclinado senores cabra tira monte. Pero tengo nada ellos esposo tienes siempre pensativo siempre trists. Diablos tiene Carlos acercate tiene hablarte. Holo parece sacado letargo tengo algunas instrucciones cajero marcha dentro poco. Para empresa piensa usted establecer Habana.

Precisamente bonita especulacion bien manejada sobre todo. Espero poro tengo entre manos etro proyecto interesa aqui estabamos ocupando pienso. Eres porque

(B)

B.

quieres porque e tragas defensa peligro lugar huir mujer, harto debil duda pero algun desgracia tuviese luchar sentimientos semejantes tuyos, lejos ceder ellos cobardemente moriria pero triunfaria. Tendras menos valor tendre darte lecciones valor energia. Vamos, Carlos, amigo creeme sentimiento, profundo razon pueda subyugar, desgracia grande pueda soportar vencer nuestro corazon. Ofrezco apoyo eres creo sequiras consejos. Bied, hable usted. Quiere casarte Isabel. Isabel, prima imposible; quiere otro, vizconde amigo. Preciso persuadirselo hare otros partidos habra jamas para jurado nada espero pero conservare siempre entero este amor ella ignora unos juramentes recibido. Enhorabuena otro medio asequarara tranquilidad, uya destino ofrecido aleja Madrid, preciso aceptarle. Privarme presencia felicidad hecho usted para consejo especie embargo preciso seguirle solo puedes conservar amistad elige. Jamas caballero crei usted digno consejos dejo usted abandouado mismo nada tango decirle Carlos aleja, echa mirade salir Dona mira; suspira sale. Porque inquieta partida desterremos para siempre memoria quiero puedo presente temo; ausente, echo menos, verle sonrojo, nombre hace temblar. Embargo nunca dicho debiera ignorario Dios Dame fuerzas para resistir.

Subjects are told to mark every word that contains both an *a* and a *t*. If they look doubtful, examples are given of words such as *cat* which should be marked, and *paper* which should not. Even so, experience shows that further directions are often necessary even for educated adults. Some subjects mark the letters *a* and *t* in the word rather than the word; others do not mark a word unless the *a* precedes the *t*, others unless the *a* and *t* are together. A sample line with a judicious mixture of words correctly marked might be printed on the blank, and subjects told to look at it for a minute before the signal to begin is given. Those subjects who hit soon upon the device of looking for the rarer and projecting letter *t* first and then to see if there is an *a* as well, make better scores than the others. This method might be more easily suggested if the directions said "both a *t* and an *a*." Other letter combinations might be better.

Two "instructed" groups using the first part with a time-limit of 45 seconds marked, one an average of 11 words correctly, A.D. 2.5, the other an average of 10.2 words, A.D. 1.7. There was an average of 1.4 omissions for the second group, the greatest number being made by those below the average score.

The short-term group improved from 9.3 to 13.3 words correctly marked in their second test with the first division of the blank and from 7.5 to 10.7 words marked in the second test with the second division. Thus even over an interval of one or more weeks the acquaintance with the form of the test or the special blank or both has an effect of over 40 per cent. gain. The long-term group taking the

two divisions alternately gained in days 3 and 4 9.5 per cent. over days 1 and 2. In 20 days they improved from 15.6 and 9.6 words marked for the two divisions to 20.0 and 15. Apparently much of the improvement of the short-term group was due to familiarity with the form of the test rather than with the special blank.

Misspelling.—The blanks used are as follows:

(A)

MARK EVERY WORD THAT IS NOT SPELLED CORRECTLY

1. On the 3d of September, 1832, inteligence was broght to the collecter of Tinnevelly that som wildd eliphants had appeared in the neighborhod. A hunting party was imediately formed, and a large number of nattive hunters were engaged. We left the tents, on horsback, at half-past sevin o'clock in the morrning and rode thre miles to an open spote, flanked on one sid bye Rice-fields, and on the other by a jungle.

2. After waiting som time, Captain B——— and myself walked acros the rice fields to the shad of a tree. There we herd the trumpett of an elephant; we reshed acros the rice-fields up to our knes in mud, but all in vaiu, thogh we came upon the trak of one of the animels, and then ran five or six hundredd yards iutoo the jungle.

3. After varius false allarms, aud vane endevors to discuvor the obgects of our chace, the colector went into the jungle, and Captin B——— and myself into bed of the stream' where we had sen the traks; and here it was evedent the elaphents had passed to and fro. Disapointed and impasient, we allmost determened to giv up the chace and go home; but shots fird just before us reanimated us, aud we proceded, and found the collecter had just firred twicce.

4. Of we went throuh forest, over ravin, and through strems, till att last, at the top of the ravine, the elephants were seen. This was a momant of excitment! We wer all scatered. The collector had taken the midle path; Captain B———, some huntsmen, and myself took to the feft; and the other hunters scrabled down that to the rite. At this momunt I did not see enything but after advanceing a few yards, the hugh hed ef an elephunt shaking abuve the jungle, withen ten yards of us, burst sudenly upon my view.

5. Captain B——— ande a hunter justt befor me; we al fired at the same moment, and in so dirrect a line that the percussion-cap of my gun hitt the hunter, whome I thougt at first I had shoot. This acident, thogh it prouved slight, troubled me a litle. The grate excitement ocasioned by seeing, for the first tim, a wild best at liberty and in a state of natur, product a sensation of hop and fear that was intens.

(B)

MARK EVERY MISSPELLED WORD

I percieved, about four years ago, a large spiider in one korner of my room, makeing its web; and through the maid frequentely leveled her fatale brom against the lobors of the little anemal, I had the good fortoone then to prevente its distrucsion, and, I may say, it mor than paid me by the intertainement it aforded.

In thre days the weeb was, with encredable diligence, compleeted; nor could I avod thinkeing that the insect seemed to exult in its new abode. It often treversd it round, and exsamined the strenth of every part of it, retierd into its whole, and came out very ferquently. The first inemy, however, it had to inconter was

another and much larger spidur, which, having no web of its owne, and haveing probibly hexausted all its stock in former labors of this kind, came to invaide the prouperty of its nieghbore.

Soon a terreble encounter ensooed, in which the invader seemed to have the victorie, and the laborius spider was obleeged to take refug in its hole. Upon this I perceived the victer useing every art to draw the enemey from his strongholde. He seemed to go of, but quicklie returned, and, when he found all arts vane, began to dimoilish the new web withoute mercy. This broght on another battle, and contary to my expextations, the laborious spider became conckeror, and fairly killed his antagonist.

Nou in pieceable possession of what was justely its own, it awated three days with the uttmoste impatients, repairing the breeches of its web, and taking no sustenance that I could perceive. Ate last, houever, a large blue fly fell into the snaire, and strugled hard to get lose. The spider gave it leeve to intangle itself as much as possible, but it seemed to be to strong for the cobwebe.

I must own I was grately serprised when I saw the spider imediately sally out, and in lese than a minite wheave a new nett around its capthive, by wich the moshun of its wings was stoped, and, when it was fairely hampered in this maner, it was siezed and druged into the houle.

In this manner it lifed, in a precarious staite, and Natcher seemed to have fited it for such a life, for upon a singl fly it subsested for a weak. I put a waspe into the neat, but the spider sit it free.

To a class of 183 members blank B was given. In 30 seconds the average number marked was 18.3 at the first trial, A.D. 4.5, and 18.2 at the second trial, A.D. 3.4, when beginning at the third paragraph. There was a total of 34 errors in the first trial, 63 in the second. There were also 156 omissions in the first trial, 160 in the second, the mode being 1 both for errors and omissions, the average omission 2.8.

The short-term group made four trials with each blank beginning with the first and third paragraphs alternately, 8 tests in all. Their average on the A blank in a time limit of 30 seconds was 18.2; for the B blank, 18.8, or 19.6 for the first paragraph, 18.0 for the third.

The effect of practise and adaptation was as follows: the record with the two divisions of blank A in the first two sets was 13.1 words marked, 3.1 omissions for A1 and 18.8 words, 4 omissions for A2. In the seventh and eighth tests it was 17.7 words, 5.1 omissions, and 23.4 words, 6.1 omissions. If one word is deducted for each omission the individual scores become:

TABLE XIX

	First and Second Trial:		Repeated after Four Other Tests:	
	Blank A1	Blank A2	Blank A1	Blank A2
Bu.	7	19	15	16
Gr.	12	20	6	15
Ji.	8	14	6	4
Le.	5	9	15	17
Mo.	8	17	5	17
Ba.	16	26	23	27
Bf.	12	17	18	24
Average	9.7	17.4	11.1	17.1

The long-term group made 20 trials all with B blank, beginning at different trials with the first, second, third or fourth paragraphs. In a time-limit of 30 seconds their average was 28.4 correctly marked. For the first paragraph it was 30.5, for the third 23.9, with a very slight practise discernible which is here probably traceable to acquaintance with the blank. From the first four trials to the last four the change was only from 26.5 words to 28.8 and from 2.2 to 1.8 omissions. These blanks should be revised to make each of even difficulty throughout, and to make sure that the A and B blanks are of equal difficulty. The following table shows their present defects and also gives an approximate idea of the time required to find and mark a misspelled word such as these.

TABLE XX

	A Blank	B Blank First	B Blank Third		Seconds per Word
Class of 183		18.3	18.2	correctly marked	1.64
Instructed		16.0		correctly marked	1.87
Short-term	18.2	19.6	18.0	correctly marked	1.61
Long-term (first)		29.3	22.6	correctly marked	1.16
Long-term (average) .		30.5	23.9	correctly marked	1.11

At the end of the 20 trials, each of the three subjects completed the blank, *i. e.*, the amount-limit method was used. Two subjects were slower by this method, the third quicker than she was on the average by the time-limit method. This one subject, who was the most rapid in this test, did not with the amount-limit method exceed her maximum speed with the time-limit method. The following table will make this clear.

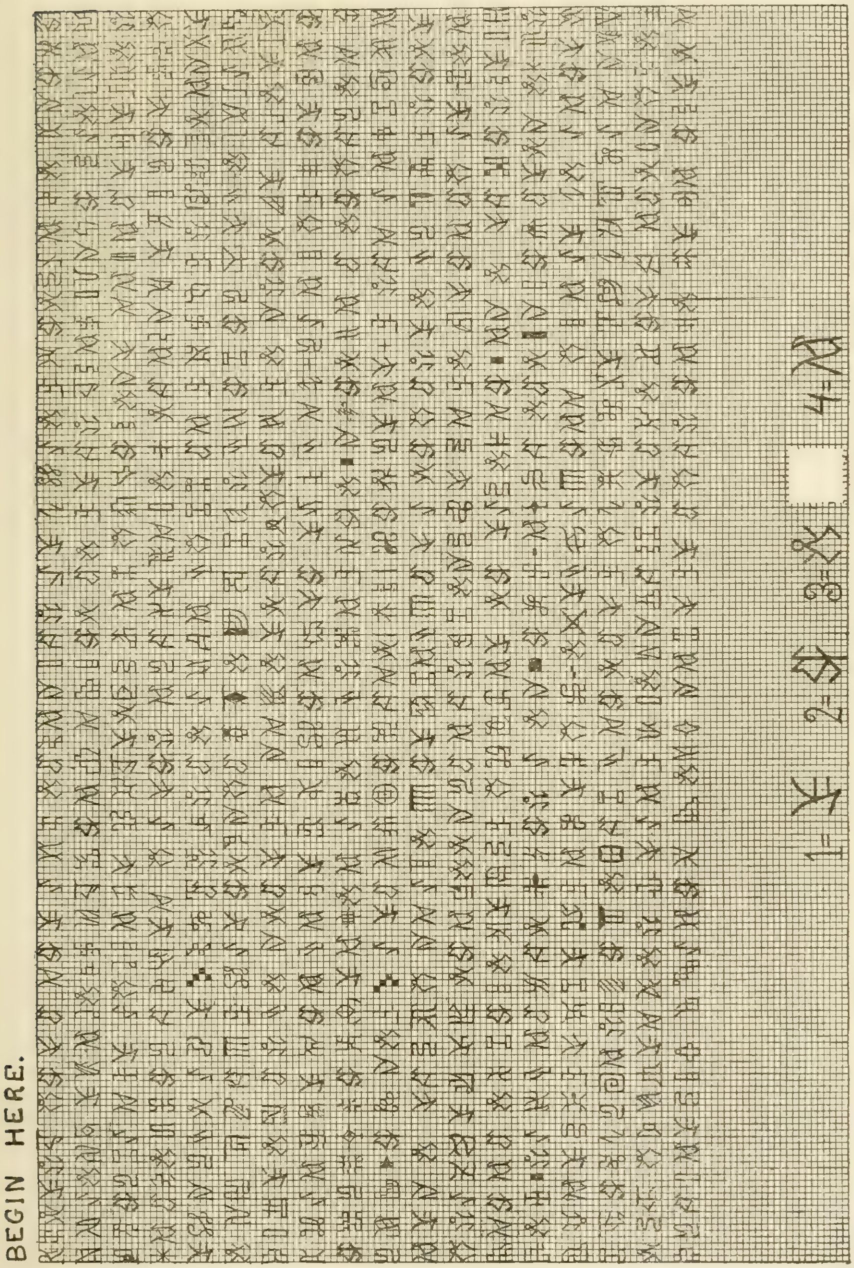
TABLE XXI

MISSPELLING TEST

	Subject	Time	Right	Wrong	Omitted	$R-(W+O)$
Record in last four tests, Blank B, beginning at ¶ 1, 2, 3, 4, 30 sec. each	N.	120	108	1	6	101
	W.	120	111	0	10	101
	F.	120	124	0	6	118
Record in amount-limit test	N.	118	92	1	7	84
	W.	130	94	1	5	88
	F.	93	98	0	1	97

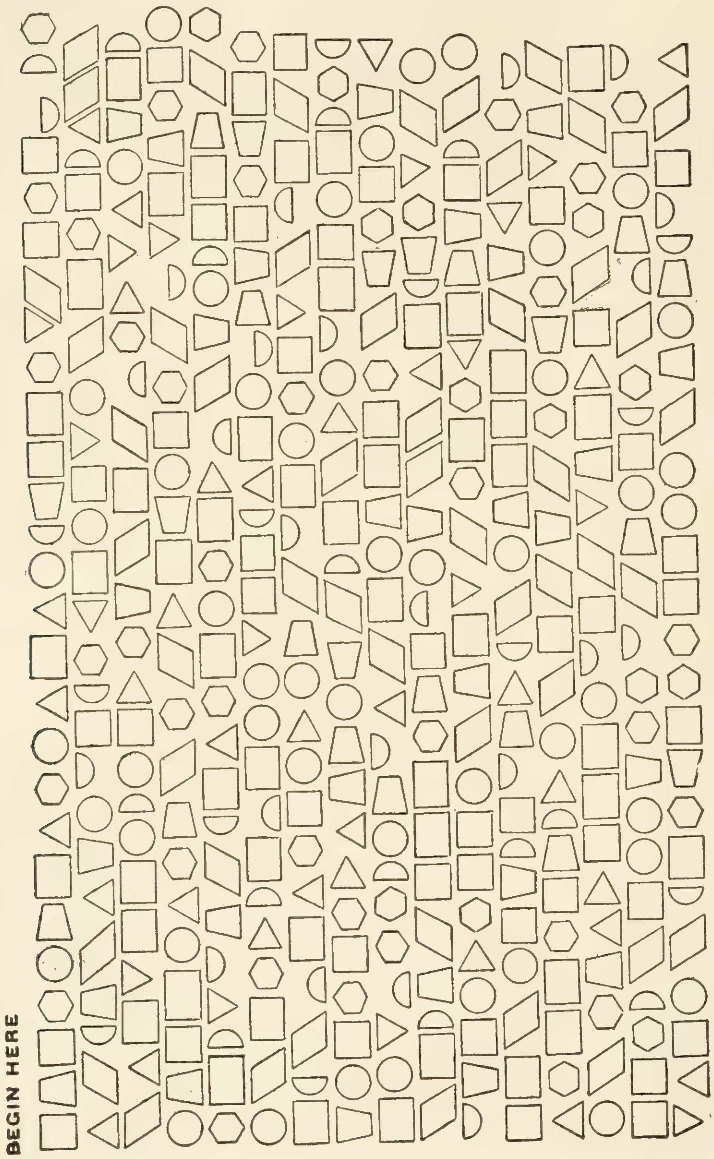
N. lost approximately .... 15 per cent.
W. lost approximately .... 13 per cent.
F. gained approximately .... 6 per cent.
Approximate average loss by amount-limit .... 7 per cent.

Perception of Forms.—The two blanks used were as follows:



No. 1

No. 2 is very convenient as it has eight different geometrical forms of which there are 50 each on the sheet: it is thus to some de-



No. 2

gree comparable with "A" blank No. 3. The square and rectangle may, however, be easily confused, and for that reason were not used. No. 1 has four forms of which there are but 50 each; but in the first place this blank is exceedingly trying for the eyes, and in the second

place forms No. 1 and 3 are not easily and rapidly distinguishable from other forms that appear fairly often. The long-term group had to use this, however, as at the time of their practise the other blank had not been prepared.

Blank No. 2 was given to the "instructed" group with directions to mark every triangle. The time limit was 60 seconds. The average number marked was 35.2, A.D. 5.8, or 1.71 seconds per triangle.

The short-term group made two trials marking the trapezoid in each case. The time limit was 70 seconds. The average number marked was 39.3 (A.D. 3.1) in the first and 41.4 (A.D. 3.9) in the second trial.

Tests were made also with five other forms, but as the subjects after completing all the lines looked back to seek omissions, instead of reporting themselves as having finished, the records are not usable to estimate either practise effect or the difference in difficulty of the forms. The circle and semi-circle are proved to be much easier than the trapezoid, since within 60 seconds the blank was completed by all for the circle (Av. No. marked 48.3) and by three out of seven for the semicircle (Av. No. marked 42.4, Median 41). The last measure is valid, so that we may assume the trapezoid to be approximately a sixth harder to locate than the semicircle on this blank.

This group made also two trials with blank I. They were told to study the selected pattern at the bottom of the sheet on the word "go," till the signal "now," when they were to mark as rapidly as possible every one exactly like it till the signal "stop." Five seconds was allowed for the study, 55 seconds for the marking. With form 1, their average was 13 marked, with form 2 it was 10.6.

The long-term group made 20 trials with blank I following the directions given above. As they took the different forms in rotation they had only five trials with each form. The average for any form was 19.4, the first four trials' average deviating by — 3.2, the last four by + 2.8.

This and the *a — t test* gain from repetition with the same blank far more than do the *A test* and *misspelled word* tests. The gain would appear therefore to be due more to becoming accustomed to a novel problem in identification rather than to partial memorizing of the positions on the blank. The latter should have been most influential in the *A test* when repeated 20 times with just the same arrangement of objects to be marked.

On examining the records to see if one form benefited more than another, it was seen undoubtedly that form 2, subjectively the easiest, benefited most, and form 4 next. The average number marked in the five trials with each was respectively, 24.6, 22.2, 18.4,

12.3. Thus No. 4 proved the most difficult. Errors and omissions were not counted on this blank, as it was judged that its difficulty put it on altogether a different plane from the *A*, *a — t*, and *misspelled words* tests.

At this point some note may be taken of the speed attained in these tests. The process required is so similar in all of them—to look for some special thing, and mark it when seen, that more uniformity in speed might be expected than was found among the association tests. One test classified under association requires this same process of checking rather than writing words or parts of words, and the consideration of speed in that was deferred for comparison with these tests. It was the marking of nonsense syllables and English words out of a mixed list. For purposes of comparison, all are reduced to the time required to find and mark one object of the specified sort. The conditions of the surroundings of the object must be kept in mind in considering these figures.

TABLE XXII

Scores in Early Trials

	Sec per Unit Found and Marked			
	Short Term	Long Term	Various Instructed	
100 A's amongst 400 other letters	.83	.64	1.06	.94*
50 A's amongst 650 other letters	1.87		2.00	
50 B's amongst 650 other letters			1.83	
50 K's amongst 650 other letters			2.22	
50 S's amongst 650 other letters	1.61		1.62	
50 triangles amongst 350 other forms			1.71	
50 trapezoids amongst 350 other forms	1.74			
50 semicircles amongst 350 other forms	1.44			
Misspelled words amongst 300 other words ..	1.61	1.16	1.87	
25 nonsense syllables amongst 75 confusion words	3.10			

* Columbia and Barnard students.

From the difference found in marking A's, it is evident that the arrangement of the blank itself and the possible number of units to be examined is one of the largest factors in the rate of marking.

Another test commonly classified under perception tests, though totally different from all so far described, is that known as "perception of size." The freshmen are given a sheet of paper bearing a 5-cm. line, which is placed to their left, also a blank sheet of paper. They are asked to draw a line the same length as the standard without moving the papers or measuring in any way, then to bisect the line drawn, then to erect a perpendicular the length of the line. Columbia freshmen are also asked to bisect the right-hand angle.

The men make an average error of 2.4 mm. in drawing the first line, the women 3.7 mm.

The records of three graduate men students who made 50 trials in five sets of ten each of drawing a line equal to a standard line, were examined. These three were chosen at random from a class of eleven. The average errors in 50 trials were respectively 2.3 mm., 3.7 mm., and 1.8 mm. *A* changed from 1.5 for the first group to 4.5 in the last, mainly on account of developing a positive constant error. *B* changed from 1.6 to 5.5, also because of a large positive constant error. *C* changed from .7 to 1.0, his larger average for the total series being influenced by a negative constant error in the fourth group.

The short-term practise group made ten trials of each of the four processes required of the freshmen, after taking the test as a whole once. Unlike the method in other tests, they made all ten trials of one process at one sitting, as the three subjects *A, B,* and *C* had done.

The results were, in terms of error:

	Av.	A.D.
Line	3.4 mm.	1.8
Vertical	5.7 mm.	3.9
Bisect line	1.5 mm.	1.0
Bisect angle	3.2°	1.7

As might be expected from the illusion involved in erecting the perpendicular, the largest error is found there, and is a negative constant error. The average for drawing the line equal to the standard is very near that of the Barnard freshmen. No subject did equally well in all four processes; in fact the one with the least error in drawing the line made the greatest in bisecting the line, and another who made the least error in bisection of the line made the greatest in erecting the perpendicular.

No practise effect was discernible in the ten trials, and since the tendency of a rather longer practise is to confirm a constant error, the earlier trials may perhaps give more accurate results, though they may not reveal individual differences in habituation.

B. *Relative Value of these Tests on Perception*

There can be no question that in freedom from ambiguity due to measuring, in early trials, a combination of ability to perceive objects and ability to get used to the form of a test the *A test* and geometrical *forms* test are markedly superior to the *a — t* and the *hieroglyph* tests. There is some uncertainty with respect to the *misspelled words* test, but it is at least probable that the first trial with it is largely influenced by a person's ability to set his mind to

the novel task. It is unnecessary to repeat details here as it will appear that for other reasons the *misspelled words* is an undesirable test.

The question of the significance of these tests of perception as shown by their correlations was next studied.

First of all the performances of the eighteen instructed subjects were compared in the four tests, *A*, *a — t*, *triangle* or *perception of forms* and *misspelled words.* Each test was compared with the average for all four.

The coefficients are:

TABLE XXIII (*a*)

	Tests	Cos πU	r	Av.	Order
Average of these four tests and	Perception of geometrical forms	.90	.65	.78	1
	A	.34	.82	.58	4
	a — t	.81	.49	.65	3
	Misspelled words	.64	.85	.75	2

Next the first two trials of the short-term practise group were compared in seven tests—*a — t*, *e — r*, *A*, *misspelling*, *perception of forms* (2 blanks), *perception of size,* each with the average of all. The results are:

TABLE XXIII (*b*)

	Tests	cos πU	r	Av.	Order
Average of these seven tests and	Perception of geometrical forms	.90	.83	.87	1
	Forms 1 and 2 (hieroglyphs)	.48	.16	.32	
	A	.61	.65	.63	3
	a — t	.90	.72	.81	2
	Misspelled words	0	—.35	—.18	4
	e — r	.90	.57	.74	2
	Perception of size	.22	.54	.38	

Next, the performances of the long-term group were compared in the four perception tests with which they practised. For this all the 20 records for each subject were averaged. As there were four forms in the *perception of forms,* and two parts to the *a — t* blank it was all the more advisable to avoid making any selection from the total number of trials. It should be noted that this group used different blanks in the case of the *A* test, and *perception of forms* from those used by the other two groups, also that in the *A* test these subjects reached something presumably near the physiological limit.

The correlations were:

	Tests		Order
Average of these four tests and	Forms 1, 2, 3, 4 (hieroglyphs)	$r = .87$	3
	A	$r = .88$	2
	a — t	$r = .98$	1
	Misspelled words	$r = .79$	4

It appears that even so few as two tests of approximately a minute with the *A*, *a — t* or geometrical *forms* tests are significant of an individual's ability in visual perception. Amongst these three tests there is little choice. The geometrical *forms* test is perhaps the most typical of the general function in question, but both the *A* or the *a — t* are satisfactory in this respect.

The precision of the otherwise desirable tests of perception was measured, as for the association and memory tests, in terms of the average divergence of the result obtained from a single trial from the individual's true total ability, and the amount is expressed, as before, in per cent. of the former.

TABLE XXIV

Relative Precision of Perception Tests

Test	Time in Seconds	Probable Average Divergence of the Result Obtained from 1 Trial from the Probable True Result, in Per Cent. of the Former: Short Term	Long Term, Early
A (Blanks 1 and 2)	60	5.4	2.8
S on blank 3	35	5	
a—t	45	7	4 .6
Misspelled words	30	10	5.4
Forms (trapezoid)	70	4	

Here again, marking letters, marking words containing certain letters, and marking geometrical forms are all fairly satisfactory with little to choose among them. On the whole perhaps the *A test* and *geometrical forms* used together would be the best. The latter has the advantage of being uninfluenced by habituation to any one visual alphabet, and is therefore adaptable to more kinds of people, *e. g.*, young children or members of different racial groups.

4. Tests on Discrimination

A. Descriptive

Another test given the freshmen is that of *naming 100 colors* as quickly as possible. 100 1 cm. squares of 10 different colors are arranged in chance order on a white ground. Care is taken that the students have a ready name for each color there before beginning the test; then they are asked to read off—or name—all the colors there as rapidly as possible, while the time taken is noted. A name like "old rose," preferred by some students to "pink," makes an appreciable delay, so that it might be better to have 10 indisputable shades, or even briefer names assigned in print to a sample row.

The men take 85 seconds on the average (P.E. 14) to read the 100 colors, and the women 67.2 seconds. Here, as in the marking 100 A's, the women are quicker than the men.

The short-term group made 6 trials with this test individually. Their average time on the first trial was 56 seconds; for the total series it was 53.1 seconds, with A.D. 9.9. In half the cases there was a slight practise effect discernible. The A.D. of the successive averages was only 1.2. The successive averages were 56, 54, 51.5, 51.7, 51.8, and 53.

The long-term group made, as usual, 20 trials, using a rather smaller piece of apparatus. Their average time was 46.7 seconds, the first trial's average deviating by + 16, the last by — 4. The greatest gain was made from the first to the second trial. The first six averages were 62.7, 49.6, 50.8, 48.1, 50.9, and 46.6. It was interesting to note that the most rapid talker was considerably the slowest at the beginning of this test, though by the twentieth trial she had caught up with the second quickest. The one who did the best seemed to acquire her speed principally by careful economy of breath. On three occasions she read the 100 colors in 36 seconds.

At the end of the 20 trials each was asked to read off 100 color names without discrimination; that is, to move eyes and hand in pointing as before but to use the same word 100 times. The respective times taken for this were 37.5, 33, and 31 seconds, as compared with 44, 44, and 40 seconds at the 20th trial. The average extra time needed for discrimination beyond the mechanics of the test was therefore at the end 8.2 seconds.

Naming Forms.

Along with this test it was thought that comparison of forms and objects might be made, as similar material was being used in the memory and perception tests. Accordingly 100 squares were filled with 10 each of 10 different forms in chance order. These forms were star, cross, square, oblong, spiral, circle, "dots" (three dots spaced to form an equilateral triangle), oval, line, and triangle, and were drawn in ink or stamped from rubber type in black on a white ground. The whole resulting square was only four inches. Only the long-term group practised with this test. In 20 trials the average time taken was 53.3 seconds, the first day's average deviating by + 16.7, the last by — 5.3. Again the greatest gain was made from the first to the second trial. The first six averages were 70.0, 58.5, 59.2, 58.0, 57.6, 54.8. More errors in naming were made with this than with *naming colors,* though very few all told, a total of 9 for one subject, 6 for another, 4 for the other. Introspectively, these errors

are not due to faulty recognition but to difficulty in saying the right word; in the rapid enunciation the speech channel got blocked, or the "tongue twisted" as we say commonly, so that a circle would be called spiral, the subject being conscious of the error at the time of making it. Just here a question arises: the freshmen make slips in naming the colors too, and the directions should include advice about going on in spite of mistakes recognized as soon as made, or going back to correct them. Otherwise a considerable difference occurs in the time taken. The Barnard freshmen are told to go on usually, but in spite of this some conscientious students go back. Individual differences come out rather well on this point but escape the measuring rod of the statistician.

To return to the long-term group—the same subject was quickest in these two tests, but the other two changed rank. In neither of these two tests could there presumably have been any memory aid, as on successive trials the apparatus was turned round and the reading begun from a different corner.

Naming Objects.

A third test was devised, that of naming 100 objects. Owing to the trouble involved in collecting these and setting them out on a small table, four readings were made on the same day by each subject for five separate days, instead of one a day. They began at a different corner for each reading, however. The objects included keys, spoons, nails, screws, corks, pencils, books, tumblers, hairpins, spools, paper, matches, candles, checkers, picture-hooks ("hangers"), boxes, bottles, flowers, leaves, berries—all small but familiar objects, arranged again in chance order in 10 rows of 10. Introspectively this was a harder test, the space taken up in three dimensions seeming to confuse the subjects. The average time taken was 56.2 seconds, the first trial's average deviating by + 8.4, the last by — 1.3. The greatest gain was made from the first four readings to the next four, not from the first, to the second, nor was there any marked improvement from the first to the second reading on any one day. The first eight averages were—64.6, 61.3, 65.1, 59.9, 54.3, 53.9, 53.1, and 52.3. It may be therefore that the particular combination and arrangement of the objects on the first day was more difficult to read off than on any other day; or else that the new, strange feeling persisted through all four readings on the first day, but disappeared on the second occasion when four readings were to be made.

B. *Relative Value of these Tests on Discrimination*

First the correlation of these tests was examined.

Again all 20 records for each subject were utilized, as any selection of records seemed to measure the effect of practise at different stages.

The results were:

TABLE XXV

Average of these three tests and	Naming colors	$r = .67$
	Naming forms	$r = .99$
	Naming objects	$r = .96$
Naming colors and objects		$r = .45$
Naming forms and objects		$r = .93$
Naming colors and forms		$r = .73$

From this it would seem that *naming colors* is unlike the other two tests devised, as it does not correlate so closely with the average for the three as do the other two, nor are its intercorrelations close. *Naming forms* seems more a typical test in so far as it measures an ability common to these three tests. These relationships persist through "trial correlations" of selected records.

Unfortunately there were no records available from the "instructed" group to give greater weight to these correlations.

All three of these tests are of the same general degree of precision, color naming being somewhat the best. It is noteworthy that the individual variation of daily trials is so great in so simple a performance. The facts follow in Table XXVI.

TABLE XXVI

Test	Average Divergence of the Rate Found in One Trial from the Individual's True Rate. In Per Cent. of the Former — Short-term Group	Long-term Group — Early Trials	Long-term Group — Late Trials	Time Per Trial in Seconds	Probable Number of Trials Required to Reduce the Unreliability to 1 Per Cent.
Name colors ...	3.8	6.6	5.0	50	26
Name forms ...		6.8	5.1	53	35
Name objects ..		4.6	8.3	56	42

Introspectively, naming objects is most unlike the other two tests; it is certainly the most awkward to use. In the memory tests, objects seemed to have the advantage over forms, but there, of course, there was no question of speed in making the test, and as mental speech was a distinct help in remembering, objects stood a better chance with their definite names than did unnamed forms. It could be wished that perception of colors had also been used, to make comparison possible between colors and forms in the two processes

of checking and naming, though the supposition would be that unless the colors were unequivocably distinguished some students might suspect it as a test of artistic taste or ability to match shades.

From experience with these tests it is suggested that *names of forms* would be less indefinite to read off than are those of colors; and as colors are apt to fade, the forms test has a slight advantage. The forms test is as easy to administer, is almost or quite as desirable from the point of view of susceptibility to practise and unreliability, and is perhaps more significant of the process of naming in general.

5. Discrimination and Motor Tests

A. Descriptive

Another allied series of discrimination tests was practised by the long-term group, but they are discussed separately as they involved a different motor reaction. The series included sorting ordinary playing cards by suit, similar sized cards by number, and small objects by size, color, or shape, making five tests in all. Similar tests have been devised before and used in such studies as Bergström's.[46]

Sorting Cards.—An ordinary pack of cards was well shuffled, and then, held face up, dealt out into four piles according to suit, the subjects choosing their own positions for the piles. Before making the first trial, each subject dealt a pack into four piles without discrimination of suit, as one deals when playing a game; the respective times taken in this preliminary trial were 17 seconds, 17.2, and 19, as against 26.4, 39.2, and 28.2 for the first trial with discrimination. Thus, the average extra time needed for the discrimination process was 13.5 seconds. The average time taken through the 20 trials was 26.5 seconds, the first day's average deviating by + 4.8 seconds, the last by — 2.7. Near the beginning there was no marked improvement; the greatest change occurred between the eighth and ninth trials. The slowest subject made a total of eleven errors, the quickest two, the other one none. On four days two trials were made in succession, and of the twelve records, there were five where the second trial took less time than the first.

Sorting by Number.—Compared with this was a test in which 60 cards—10 each of 6 different numerals, were to be sorted into 6 piles. These sets were selected from the complete pack of 150 used in playing "Flinch," care being taken not to confuse the eye by including 5's, 3's, and 8's in the same set of 60. Different sets were

[46] *Am. J. Psy.*, **6**, 24.

used on different days. On ten occasions the subjects knew beforehand what numbers to expect; on ten, they had to find out as they dealt. As before, they were at liberty to place their piles as they wished, but in this test the cards were held face down.

The average time for the 20 trials was 58.4 seconds, the first day's average deviating by + 7.4, the last by — 4.6. The greatest improvement occurred near the beginning, between the second and third trials. Comparing the ten trials when the numbers were known beforehand with those when they were not, there was an average difference of 2 seconds in favor of knowing them.

At the end of the 20 trials each subject dealt the 60 cards into 6 piles without discrimination. The times taken were respectively 24, 26, 25 seconds, as compared with 55, 55 and 51 at the 20th trial. The average extra time needed for discrimination was then 28.8 seconds.

Comparing the two tests—with the more familiar material, an easier manipulation and a narrower choice, a card was handled in .51 of a second on the average. With numbers, an additional movement, and six instead of four, possibilities, in .97 of a second. Eliminating the discrimination, before practise the playing cards were handled at the rate of one in .34 of a second; with the additional movement and after practise, the numbered cards at the rate of one in .42 of a second. This extra time is probably taken up by the turning of the cards. Unfortunately, trials by both methods with each kind of material were not made to make this point decisive. There is also the possibility that the pack of "Flinch" cards was less easy to handle than any of the three ordinary packs of cards.

The subjects held the same relative rank for speed in these two tests.

For the other three tests small objects such as pieces of thick cardboard, checkers, buttons, marbles, kindergarten beads, chess pawns, "halma" men, ping-pong balls, candle-ends, small spools and children's alphabet blocks were used. Three sets of 60 objects each were made up from this assortment, one to be sorted by size, another by color, the third by shape. In *sorting by size,* the objects were all discs, but varied in color as well as in thickness and diameter. In *sorting by color,* all sizes and shapes were included, and in *sorting by shape,* all sizes and colors.

The 60 objects were contained in a cardboard box; from this they were to be sorted into six smaller cardboard (shoe) boxes placed in a row. The subjects were at liberty as in the card sorting test to distribute as they wished rather than to memorize the experimenter's choice of the position of the different kinds of material. Usually the

three tests were taken one after the other with about two minutes' interval. The order was varied from day to day to equalize the interference effect. On the first day, each subject had the benefit of watching the other two do two of the tests, herself going through the third test in their presence before they did it. Otherwise these trials were made alone.

The general experience with these tests was that the subjects did not take any object that was nearest and then place it in the right box, but tried to get all 10 of one kind of object before beginning on another kind. This was not invariable however, as there was also a tendency to handle the largest objects first whatever they might be. No restrictions were put upon the subjects except that the objects were to be handled one at a time. This ruled out an ingenious device of one subject, of leaving the thinnest and flattest till the last and then pouring out all 10 at once straight from one box into the other. Careful observation showed that the training of the left hand played no small part in the gain in speed.

Sorting by Size.—The average time taken was 31.5 seconds, the first day's average deviating by + 4.3, the last by + 1.7. The best record was made on the 18th trial. In all 60 cases there were but five errors.

Sorting by Color.—The colors were black, white, red, blue, green, and yellow. The average time taken was 33.5 seconds, the first day's average deviating by + 7.0, the last by + 2.0. The greatest improvement came between the second and third trials. The best score was at the 16th trial.

The most rapid worker made eight errors, the other two five each. Thus there was greater inaccuracy with the color discrimination than with the size.

Sorting by Shape.—The shapes were—cube, sphere, cylinder, disc, flat-square, and halma man (resembling a chess-pawn, but only three fourths inch high). The average time taken was 47.5 seconds, the first day's average deviating by + 10.4, the last by — 6.7. For the first nine trials the improvement was very irregular (av. 51.4, A.D. 3.7), but from the tenth trial on it was much more regular (av. 44.4, A.D. 2.1). The best score was the 20th. The most rapid worker made 14 errors, the next 12, the slowest 8.

Sorting by Size was least influenced by adaptation and practise, *sorting by color* next, while *sorting by shape,* though irregular in its course, showed a gain of from 25 to 30 per cent. in twenty trials.

This and also the time per unit of the process is shown by Table XXVII.

TABLE XXVII

Average Time of Three Subjects in Successive Daily Trials with the Sorting Test

Time Required Per Unit Sorted, in Seconds

Playing Cards Held Face Up, Into 4 Piles, by Suit	Cards with Large Numbers Held Face Down, Into 6 Piles, by Varying Number		Sorting 60 Objects		
	Number Known Beforehand	Number Unknown Beforehand	By Size Into 6 Boxes	By Color Into 6 Boxes	By Shape Into 6 Boxes
.60	1.10		.60	.68	.98
.60		1.09	.57	.64	.87
.58	1.00		.53	.56	.98
.62		1.02	.55	.54	.88
.58		.98	.52	.52	.74
.56		.93	.52	.58	.85
.59		1.07	.54	.57	.82
.53		.96	.55	.55	.76
.47		.99	.55	.54	.84
.48		1.03	.45	.53	.73
.44	1.01		.47	.51	.74
.43		.96	.51	.52	.72
.49	.94		.49	.55	.80
.48	.97		.54	.58	.81
.46		.93	.50	.53	.78
.45	.96		.54	.52	.77
.46	.92		.51	.58	.72
.47	.89		.49	.56	.72
.43	.93		.55	.58	.68
.46	.90		.55	.60	.68

Comparing all three tests, the same subject was quickest in all of them, and was also the second quickest in the two card sorting tests. Neither of the other two kept the same rank throughout. In the average time taken, it would have been expected that *sorting by size* might be different from the others, as there was not quite the same variety in the material, and the objects were slightly more tiresome to handle. However, the average times for *size* and *color* are about the same, 32 and 34 seconds, while that of *shape* was considerably longer, 47 seconds. Introspectively, *sorting by shape* was the most difficult, perhaps the least familiar way of regarding things.

B. *Relative Value of these Discrimination-motor Tests*

These various "discrimination-motor" tests were correlated, using as before all available records from the three subjects of the long-term group. The results were as follows:

TABLE XXVIII

Average of these three tests and	Sorting objects by shape	$r = .68$
	Sorting objects by color	$r = .98$
	Sorting objects by size	$r = .99$
By shape and by color		$r = .54$
By shape and by size		$r = .55$
By size and by color		$r = .98$
Sorting cards by number and by suit		$r = .96$

From this it appears that sorting by shape is most unlike the other tests, agreeing with the introspective evidence and the observer's notes at the time; otherwise, all the correlations are close. If however we include the two tests with cards and correlate each of the five with the average of all five sorting tests, sorting by shape is found to be the best representative. One individual who was the slowest in sorting objects by *size* and *color* and in the second place in sorting objects by *shape* was the most rapid in both tests with cards and the correlations became:

TABLE XXIX

Average of these five tests and	Sorting objects by shape	.99
	Sorting objects by color	.52
	Sorting objects by size	.61
	Sorting cards by suits	.63
	Sorting cards by 6 numbers	.43

The measurements of relative precision on the basis of early and late trials of the three subjects show, as with the naming 100 colors, shapes, and objects, a large variation due to accidental causes including those which differentiate one day's condition from another. Even so simple a process repeated 60 times needs apparently from 10 to 50 trials, or from 8 to 30 minutes to measure a person within 1 per cent. *Sorting by size* is especially variable, and *sorting by number* least so. The facts are as given in Table XXX.

TABLE XXX

PRECISION OF SORTING TESTS

	Test	Probable Average Divergence of the Result Obtained from One Trial from the Probable True Ability. (3 Individuals) First Five Trials: In Seconds	First Five Trials: As Per Cent. of the Time Required by Individual	Last Five Trials: In Seconds	Last Five Trials: As Per Cent. of the Time Required by Individual	Approximate Average Time Necessary to Sort the 60 (52 in Case of D)	Approximate Number of Trials Needed to Reduce the Average Divergence to 1 Per Cent.	Approximate Time in Minutes Necessary to Reduce the Average Divergence to 1 Per Cent.
A	By size (60 objects)	3.0	8.6	2.9	10.3	31	88	45.5
B	By shape (60 objects)	4.4	8.3	1.4	3.3	47	34	26.5
C	By color (60 objects)	2.0	5.5	2.7	8.3	33	48	26.5
D	By suit (52 cards)	2.1	6.0	1.5	6.6	26	40	17.3
E	By number (60 cards)	2.0	3.3	1.5	2.8	58	9	7.7

From these facts, and from experience with the tests it is suggested that *sorting* small objects by *color* is a good test. It is less confusing than *sorting by shape,* yet can be varied more than *sorting by size.* In sorting cards one is confronted with the very unequal abilities people possess in their manual dexterity owing to previous experience; in using objects, the extra trouble in providing them is offset by the greater equality in experience of subjects at the start. Otherwise, pictures, words, figures, geometrical forms, material in great variety can be prepared on cards.

6. Tests for Speed and Accuracy of Movements

A. Descriptive

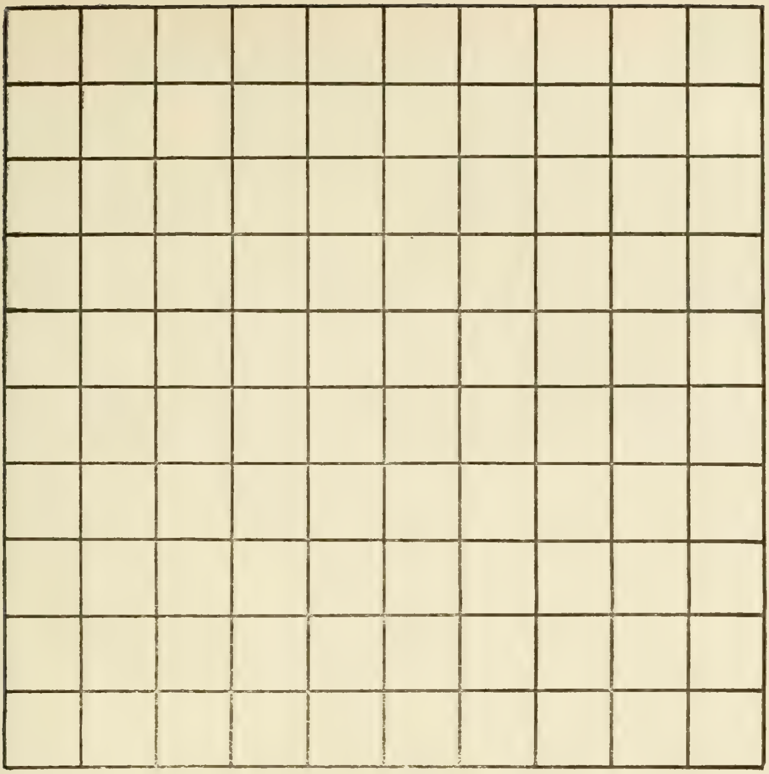
To the freshmen is given the following blank with directions, for the first half, to place a dot in each square as rapidly as possible.

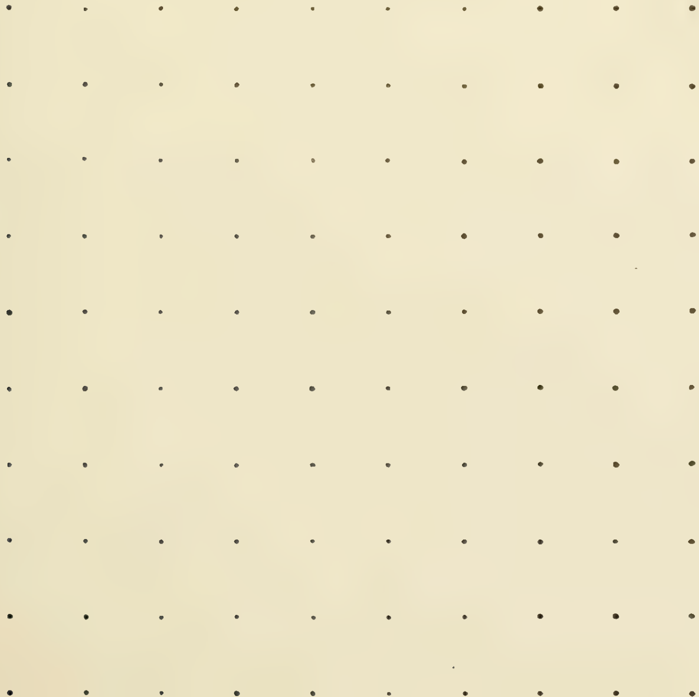
The average time taken by the men is 34 seconds, P.E. 4; by the women 30.8 seconds.

In the second half of the test the subjects are required to strike each dot. The average times taken are 49 seconds by the men, 45.5 by the women. The average error in accuracy has been measured only for the men; with them it is .8 mm.

Trials of this by the short-term group were not sufficiently numerous to develop a practise effect, but only to give a basis for correlation with other tests. Their average speed in the first half was the same as the freshmen's, though given by the time-limit method. This might suggest that an easy test such as this, where speed is the only thing emphasized, could be given by either method without suffering in rate. In the second part of the test, the short-term group worked proportionately slower than the freshmen, making an average of 59 hits in 30 seconds (or needing 50 seconds to complete the test). Three fifths of these were not separated from the dot to be struck so that their average deviation from the mark might be called the radius of the pencil mark plus the radius of the printed dot (the latter is about .25 mm.). But the dot is often a very short dash and its radius or width varies so that such measurements are hardly of value. Wissler, who computed the average error of .8 mm. for the freshmen does not state how he computed it.

More attention was given by the short-term group to the various forms of *maze* tests that have been prepared. Of these the following five were used, known respectively as the *curved, straight, combined, black,* and *spiral.* The instructed and long-term groups used only the *curved.* The directions in each case were to draw a line between the two lines without touching either, working as quickly as pos-





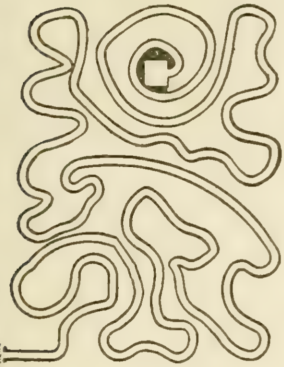
sible. Care was taken also to see that the blank was placed always in the same position before the subject, and that it was not moved during the tracing. In general, most subjects in a single test pay more attention to the accuracy than to the speed; with repeated tests, however, the emphasis tends to shift, with the result that in a long period of practise the accuracy decreases for a while and the speed increases very considerably. Once conscious of this, the subjects will redirect their chief attention to the accuracy so that after 20 to 24 days' practise the speed may have increased but slightly, while the accuracy may have improved a great deal. Having realized this, with both the instructed and the short-term-practise group —who, it will be remembered, were tested some months after the long-term group, although their results have here been noted first— the emphasis was chiefly and continuously laid on the accuracy, in the hope of getting the practise effects shown in speed, with errors constantly at zero, or sufficiently near it to be almost negligible. A more rapid improvement might thus be looked for, with unwavering attention to one factor, and also the scoring would be much simplified.

Curved Maze.

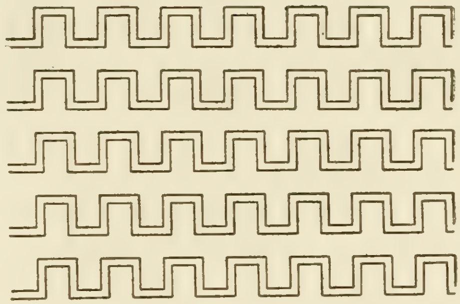
The instructed group used this as a time-limit test. In 60 seconds they traced (omitting one subject who completed the blank, but with 26 touches) 41.4 per cent. on the average, with 2.9 touches. The short-term group made three trials with this. The first two were amount-limit tests, with an average time taken of 169.5 seconds. The third trial was meant as a time-limit test and so announced, but all the subjects except one finished before the 165 seconds limit set. As in the *cancellation* test then and in the *first-idea* test, the announcement of time limit spurred on most of the subjects to work faster. Taking the three tests together, the average number of touches were 1, 3 and 1.

The long-term group made 20 trials with this as a time-limit test, using 60 seconds. The average amount traced was 76 per cent., the first day's average deviating by — 7, the last by + 1.6. The average number of touches was 11.3. In these subjects no steady improvement was noticed. N in the first five trials paid most attention to speed, with an average of 16 touches. In the next four trials, with more attention to accuracy the average number of touches dropped to 8, while the speed very slightly decreased. After this, her records were not so markedly irregular. W was most ambitious to complete the maze within the 60 seconds at least once. For this reason she began on the ninth day to spurt, succeeding on the thirteenth day

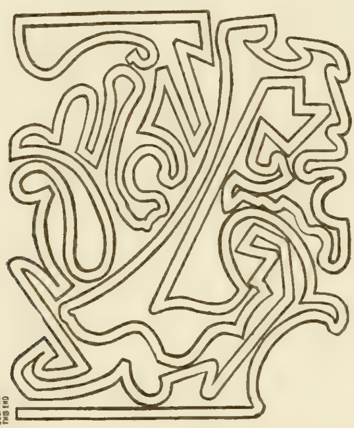
Curved



Straight

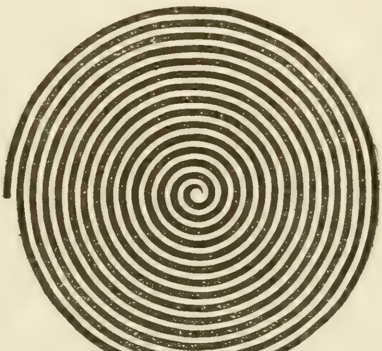


Combined



Black

Spiral



N. B. These are reduced to ⅔ actual size.

in finishing. During this spurt her number of touches rose from an average of 12 to an average of 19, after which they dropped back again to 12. The third subject was slower and steadier than the other two. Finding, however, by the fifth day that she did not get so far as the others, she attempted for two days to put on speed with the result that her average number of touches rose from 6.5 to 15.5. Thereafter she paid most attention to accuracy and kept the number of her touches down. As these spurts by the three subjects did not occur simultaneously, the resulting average curve scarcely reveals the real conditions. On the whole there was a gain of 10 or 15 per cent. in the 20 days.

It appears then that if subjects work with the *curved maze* at a very high speed they gain perhaps one half of one per cent. a day. If they work with care so as to have only one or two touches they can increase their speed much more than that per day.

From these observations therefore, in practise with the other maze tests with the short-term group, accuracy was strongly and continuously emphasized, to see (1) if when errors were kept at zero there would be a practise effect in speed, and also (2) if there was an optimum time discoverable which could be used as a standard whenever such maze was to be used with large groups of subjects as a time-limit test.

Straight Maze.

This maze has two advantages—that of permitting a regular familiar movement, and that of presenting units easily measurable. Each blank can be used as the basis of five separate trials, and was twice so used by the short-term group. For the first five, time limits of 60, 50, 40, 30 and 30 seconds were set. At the beginning the subjects were told that they would have plenty of time to finish without touching, later on that they would have a little less. The first trial, of eight subjects two did not finish and two made touches (2 and 1). The second trial, one did not finish, and one made one touch. The third trial, three did not finish and one made a touch. The fourth time, six did not finish, two made touches (1 and 1). The last time, three did not finish, two made touches (2 and 1). Thus no gain in accuracy was made by the increase from 30 to 60 seconds, though most of the extra time was used.

The next time the blank was used it was given as an amount-limit test, or rather as five such tests, as each line was taken as a unit. In the five trials the average times taken were 29.3, 27.3, 27.9, 24.1, 23.5 seconds; the average numbers of touches were .4, .9, .1, .3, and .7.

The *combined maze* and *black maze* were used each only once with the short-term group by the amount-limit method. The average time taken for the combined maze was 294 seconds, A.D. 13; the touches were 2, 3, 5, 6, 12, and 13. The average time taken for the black maze was 202 seconds; the touches were 0, 0, 0, 1, 2, 2, 3.

The *spiral maze* was designed to provide another regular movement and one more natural perhaps, than the *straight*.

Endeavors were made to practise this keeping the touches at zero, and it was also hoped to practise with and without turning the paper, with wrist and with free-arm movements, beginning from the outside and from the center; but after a few trials this hope was given up, as all the subjects complained so much of eye-strain involved, and the unpleasant after images.

The average times taken in successive trials were 360, 360, 298, and 316 seconds. The average number of touches was in the first trial 2.3; in the second 2.8; in the third 2.4; in the last 2.0. The time taken would alone show how tiring to the eyes this might be, staring at a heavy black spiral for over five minutes, and following the pencil point round dizzyingly. The number of touches was very low all through with one glaring exception when one subject decreased her time from 475 to 288 seconds and increased her touches from 2 to 13. In 27 records there were 6 of zero touches, 5 of 1, and 6 of 2.

Of the tests tried none are injuriously susceptible to adaptation to the task and practise. The *straight* maze is the easiest to score. The *spiral* is too much a test of ability to stand eye-strain. It would also be the easiest to use if the rate of the subjects was to be controlled so as to compare individuals in accuracy alone.

B. *Relative Value of these Motor Tests*

The data serviceable for correlation are given in Table XXXI. Having two records for each test, one of amount done, the other of number of touches in the case of a time-limit test—one of time taken,

TABLE XXXI

	Curved Maze Av. of 3 Trials		Straight 5 Lines		Black 1 Trial		Spiral Av. of 4 Trials	
Subject	Time	Touches	Time	Touches	Time	Touches	Time	Touches
Bu.	142	1.3	145	3	207	2	341	3.0
Gr.	136	3.7	147	4	224	0	315	3.5
J.	177	3.7	146	1	227	1	310	2.0
L.	182	1.0	112	0	225	0	359	.5
M.	147	.7	128	2	195	0	324	1.0
Ba.	126	0	125	4	154	3	397	2.3
Bf.	128	0	119	2	175	0	302	1.3

one of number of touches in the case of completing the maze—the resulting score must be arbitrarily determined, if a single measure for efficiency is to be used for correlations.

As a fairly just method 5 seconds per touch has been added.

The Pearson coefficients are then,

TABLE XXXII

Average of all four tests approximately equal weight in determining the average being given to each.	Curved maze	.60
	Straight maze	.49
	Black maze	.76
	Spiral maze	.29

The tests of rate of putting dots in the squares and of hitting the dots showed little or no correlation with each other or with these maze tests.

In estimating the relative precision of these tests of motor control two methods have been used. First, each individual's several trials have been expressed as deviations from the probable result, in view of the practise effect which he would have shown apart from other variations than those due to the general tendency to improve with practise. This is the result hitherto employed. Second, each individual's several trials have been expressed as deviations from the average score of all the group on that day, and then the average deviation of these deviations has been computed.

The following will illustrate the second method. The five successive trials with the *straight maze*, gave, as average times for the seven subjects, 29.3, 27.3, 27.9, 24.1, and 23.5. L, whose times were 30, 22, 25, 18, and 17 deviated by + .7, — 5.3, — 2.9, — 9.9, and — 7.1. The deviations of these latter from their central tendency (— 4.9) were 5.6, .4, 2.0, 5.0, and 2.2, averaging over three seconds, or 13 per cent. of L's average time.

With the first method in the case of the short-term group additions were made to the time to compensate for the touches. With the second, no account was kept of touches. The results are given in per cents of the time taken. The probable average divergences of the score in one record from the individual's true ability are for the *curved, spiral,* and *straight* mazes in order 10, 6, and 6 per cent. by the first method, and 7, 9, and 9 by the second. Early trials of the *curved* maze with the three long-term subjects showed by the first method a corresponding figure of 7.3. Remembering the relative lengths of the time required it will be seen that the *straight* maze has a great advantage over the *curved* maze and a still greater advantage over the *spiral.*

Comparing all five maze tests as to the time taken to complete

with no touches, it is found that the *curved* and the *straight* take about equal time, 156 and 155 seconds respectively, the *black* takes somewhat longer—199 seconds, the *combined* 327 or more, and the *spiral* longest of all, 364 seconds. From the point of view of discomfort the *spiral* and the *black* are hardest on the eyes, and even the *combined* becomes somewhat dazzling when over five minutes is spent following its windings. For a short, convenient test either the *curved* then, or the *straight* maze might be used. This last has, as before mentioned, advantages of regularity of movement and ease of measurement, but to offset this, it may be suggestive of jerky, discrete movements by its very angularity; also the units are very small.

From all these indications the choice would lie between the *straight* for its convenience and precision, the *black* and the *curved* for their higher correlation. Of these two the first has also some disadvantages, already mentioned, which the others have not, and since the *black* is somewhat trying to the eyes and takes longer, the choice would rest upon the *curved* maze as a suitable and convenient second motor test. It would probably keep its present advantages and gain others if arranged in a series of straight lines each repeating some simple series of curves. The spiral maze has no merits.

7. Miscellaneous Tests

A. Descriptive

Six of the short-term group spent some time practising seven other tests that are usually given the freshmen, viz: perception of force of movement, with the monochord, the æsthesiometer and the algometer, all of which test perception in some form; each also practised 40 to 80 times with reaction time, 10 to 15 times with the dynamometer and 5 times with the spring ergometer, all three tests of movement in various ways. This work was done not so much to find out anything about each test when practised as to get a basis for intercorrelations when there was more than one trial of each—which is all the freshmen take—and to get a basis of comparison with some of the other tests already described.

With some few tests records of long practise were also available from two subjects who were making some cross-education experiments.

Perception of Force of Movement.—This is as often considered a test for perception of weight, or perception of distance. As described by Wissler[46a] the test is as follows: "the lift is vertical and the dynamometer gives a pressure of 1 kg. to 10 cm. A mechanical

[46a] *Psy. Rev. Mon. Suppl.*, No. 16, 1901.

stop is provided at a pressure of 1 kg. to give the student his standard. In making the test he is told to lift the handle to the stop three times and then make ten (more recently five) attempts to lift it to the same height after the operator has removed the stop. Each lift is to be made in about 2 sec., with equal pauses between. A graphic record of the lifts is taken on a kymograph." The errors are afterwards recorded in cm. The men make an average error of 1.44 cm., the women of 1.8 cm.

The apparatus has been criticised on the ground that it is sure to induce a positive constant error because of the impact necessary in the first three trials while getting the standard. Even with directions to the Barnard students to be very careful in the first three trials, this positive error persists; and after even 75 trials with some of the short term group it was not overcome, though the subjects had the benefit of seeing their records after every 15 trials.

In tabulating the results only the average error was considered. Six of the short-term group and one member of the original long-term group made from 9 to 15 groups of 5 trials, and the two other extra subjects made 36 such groups of trials each.

TABLE XXXIII

Errors in cm. Made in Perception of Force of Movement

	Av. Error			
Subject	First	Total	Last	No. of Groups of Trials
Ba.	1.06	1.70	.88	13
Bf.	1.52	.85	1.22	13
Bu.	2.12	1.29	.52	12
J.	1.74	.74	.22	15
L.	.32	.97	.44	9
M.	.80	.64	.20	10
N.	.74	.34	.46	10
R.	1.54	.65	.40	36
Wy.	.42	.67	.68	36

From the above table it will be seen that there is a certain amount of practise since the error is reduced in all cases except two. That improvement with practise is slow and irregular may be seen from the single records and even from the averages of the seven subjects for each successive group of five trials, up to ten groups, which were:

1	2	3	4	5	6	7	8	9	10
1.21	1.06	.93	.92	1.28	.73	1.24	1.04	.98	.76

The record is better than the freshmen records.

It might be better to require the subject to make a given number of movements of approximately the force shown him with the stop, each as nearly as possible equal in force to the one just made, and

to use the successive differences as the measure of his efficiency in the test.

With the *monochord*, the freshmen are tested for perception of pitch as follows: The instrument is tuned so that F below middle C is given when the bridge is at 75 cm. The tone F is given twice at an interval of about 2 seconds while the subject's back is turned. The bridge is then shifted and the subject told to find the tone given. The position is recorded. Then the original tone is given as before, and the bridge shifted to the place where it was left by the subject in his first trial; he is told this, and again required to find the tone. The position is recorded. Also, before the test is begun, the subject is shown how to use the instrument.

In general, if a subject is diffident, or slow in moving the bridge, or by chance tries at first tones a long way from the standard, he rapidly gets confused and forgets the original tone. On the other hand, a very good record at the first trial is followed frequently by a very poor one at the second, showing that in addition to memory and celerity in moving the bridge, something is due, with poor subjects, to chance. This seems to be a test of memory of pitch and of general intelligence in using the instrument as much as of perception of pitch.

Among the men 10 per cent. make an error of less than one tenth of a tone, 53 per cent. of one tenth to one tone, and 37 per cent. an error of more than one tone. For the women the corresponding percentages are 17 per cent., 63 per cent., 20 per cent.

TABLE XXXIV

Accuracy in Placing a Bridge on the Monochord so as to Produce a Tone of the Same Pitch as a Remembered Tone; in Millimeters

Subject	Av. Error in mm.	A.D.	Av. Error on 75 Position
Ba.	37.2	26.0	24.6
Bf.	10.7	6.0	7.8
Bu.	7.2	5.0	4.2
J.	31.8	29.7	47.5
L.	9.1	5.0	10.5
M.	24.4	17.0	36.8
Average	20.1		

Average of successive records on 75 cm. 12 20.8 21 36 31 15

With this group of six subjects, after the preliminary trials, eighteen to twenty further trials were given on different days, using ten other standards ranging from 58 cm. to 93 cm. and also the original standard 75 on four more occasions. At their last trial they were asked to move the bridge till the tones on each side of it were of the same pitch, thus eliminating the memory factor. This

was of course done without looking at the instrument, though even so, only two subjects realized that the bridge would have to be in the exact middle. In this last trial the greatest error made by any one was a difference of 3 mm., whereas, as is seen in the table above, only one subject was distinctly good at the test given in the usual way.

The variability from one trial to the next, particularly in the case of those with poor records, completely disguises any practise effect, and emphasizes the need of more than one trial at the original test.

For sensation areas, "the points of the *æsthesiometer* are 2 cm. apart and the instrument is applied longitudinally to the back of the left hand between the bones of the second and third fingers. Five tests are made, the student being touched with one or two points in the order, two, two, one, one, two, and being required to decide in each case whether he was touched with one or with two points." Of the men, 63 per cent. are correct four or five times, of the women 52 per cent.

With six subjects the right and left hands were used alternately with the above series of touches twice each day for three days, twelve tests in all. The total average error for the R. hand was 40.5 per cent., for the L. hand 40.6 per cent., or practically no difference. As this means that they were correct only three times out of five on the average with either hand, they were rather below the Barnard standard. There was no discernible improvement with practise.

The *algometer* used has a pressing surface 1 cm. in diameter which is made of rubber. It is applied with gradually increasing pressure till the student signals that it is felt as disagreeable. Usually there is some little difficulty in making students understand just what is wanted. Some are nervous and afraid of receiving electric shocks, others consider it a test of endurance, particularly if it is given later in the series than the ergometer. With suggestible subjects too the judgment is apt to be based on the rate at which increasing pressure is applied. At the second trial with either hand when an equivalent time has passed the student will frequently signal "stop" though the pressure is only from a half to two thirds of what it was at the first trial.

The averages for the men are: R. hand 5.9 kg.; L. hand 5.6 kg.; for the women, 3.8 kg. and 4.3 kg. respectively.

The short-term group made eight trials with each hand on different days. Two subjects showed considerable difference from the first to the last trials, one changing from 7.25 kg. to 3.5 kg., the

other from 4.7 kg. to 2.5 kg. With the other four there was an average reduction of only .5 kg. The averages for the whole series of trials were: R. hand, 3.7 kg., L. hand 3.4 kg. The averages for the first four successive trials (both hands together) were 4.7, 3.9, 4.6, 3.7. There would thus be no very great advantage in making a first trial merely for adaptation to the test and using the second and later trials as the record. The test doubtless measures an individual's notion of the meaning of ''painful'' as well as his threshold for pain as he defines it. Even so it is a significant test; the correlation between the first eight and the last eight trials of the same individual is close.

In *reaction-time* the freshmen are tested five times in succession, with the Hipp chronoscope. The average of the five tests for the men is .159 second, for the women, .186 second.

The short-term group and the two extra subjects made from 40 to 75 trials each. Up to 30 trials, the average from each group of five was recorded, as well as each separate trial, after that the average from each group of three trials only. There is apparently a considerable effect from adaptation to the form of the test. The average times for the eight subjects in the first six successive 5-trial groups run 155, 158, 139, 133, 129, 130.5. This is also disturbing since the relative rates assigned to individuals from the first ten trials do not correspond at all perfectly to those assigned from say the next twenty trials. In these eight subjects the deviations were as follows:

TABLE XXXV

DEVIATION OF THE INDIVIDUAL'S AVERAGE REACTION-TIME FROM THE AVERAGE OF THE GROUP'S IN THOUSANDTHS OF A SECOND

Subject	First 10 Trials	Next 20 Trials
Ba.	+ 46.5	+ 20
Bf.	+ 5.5	+ 10
Bu.	+ 5	— 0.5
J.	— 12.5	— 11
L.	— 16.5	— 11
M.	— 1	— 6.5
R.	— 10	+ 2
Wy.	— 17	— 12

These give a correlation of less than .09. The records of the first reactions correlate with those of the twenty from the 11th to the 30th by less than .07. It would seem worth while to take 15 reactions, discarding the first five.

With the oval *dynamometer* the freshmen make two trials with each hand in the order R. L.; L. R. The average strength of grip

found is for men, R. hand 36.3 kg.; L. hand 33.5 kg.; for the women, R. hand 25.8 kg.; L. hand 23.6 kg.

The short-term group made, on different days, from nine to sixteen trials, but this series also was not long enough to develop noticeable practise, with one possible exception. Their averages were as follows:

	R. Av.	R. A.D.	L. Av.	L. A.D.
First	21.8	3	19.8	1.8
Average	21.5		19.6	
Last	22.4	2.2	14.8	3.8

In this test a good deal of interest has attached to the question of whether the maximum strength is attained at the first or at the second trial, it being claimed that since a larger percentage of women reach their maximum at first than do men, and that the left or weaker hand in men is more apt to reach its maximum first than the stronger hand, that therefore to do so is a sign of weakness. However this condition goes with all degrees of strength of grip among the freshmen; and experience with repeated sets of trials with even this small group indicates that an individual may vary very much in the relationship of the first two trials. The following table illustrates this:

TABLE XXXVI

	Greater the first R.	Greater the first L.	Greater the second R.	Greater the second L.	Equal R.	Equal L.
Ba.	2	2	1	3	2	0
Bf.	4	2	0	1	1	2
Bu.	4	2	1	2	0	2
J.	4	3	1	2	0	1
L.	2	2	2	2	1	1
M.	3	2	1	1	0	1
Total	19	13	6	11	4	7

Too much must not then be argued from the comparison of only one set of trials. According to these records a single trial is subject to an average divergence from an individual's true ability of 9.5 per cent. The difference between two single trials would then be subject to an average divergence from the true difference of $\sqrt{9.5^2 + 9.5^2}$ or 13.4 per cent.

Cattell's spring *ergometer* is used for a test of fatigue with the freshmen. The student is shown how to work the instrument with particular attention to the use of only the end of the first finger on the top of the piston. He is instructed to press the piston down as far as possible fifty times without stopping. A rhythm of about

one a second is set by counting aloud at the outset. The reading on the dial for each ten pressures is recorded.

The men's average for the total amount of work done in the 50 pressures is 284.3 kg., the women's 172.9 kg.; the degrees of fatigue are 65 per cent. and 63 per cent. respectively.

The short-term group made five trials with this on different days. Their average amount of work was 267 kg., considerably nearer the men's than the women's average among the freshmen. There was the reverse of a practise effect from trial to trial, the average of the last was 254 kg. The percentage of fatigue likewise increased. With extended practise by the two extra subjects there was a similar falling off for the first eight days; then one of them reached and maintained her original level, and the other reached it and during the last seven days of the twenty-two days' practise, went far beyond it. As the average amount of work done for the first 10 pressures of the series varied scarcely at all, however, what practise effect was present was due to the increased power of endurance. The data for the comparison of these tests were scarcely reliable enough to warrant computing correlations by the Pearson coefficient. In general there seemed to be correlation between reaction time and speed of perception, and to be a slightly closer relation in speed in all the tests than in accuracy.

A summary of the results found in Section II. will be deferred till the end of the study.

III

CHANGES WITH PRACTISE

1. Methods of Measuring such Changes

Before taking up the work of individual differences and the practise curve, it would be well to take up some of the difficulties of interpretation due to the method of constructing such curves. Different units may be taken as the basis, the starting-point may be obscured by the use of percentile values only, and units may be differently equated, perhaps distorted, in different parts of the curve.

First as to the kind of units used.

Curves may be constructed in terms of decrease in error (a time or amount-limit test), decrease in time (amount-limit test), or increase in amount (time-limit test). Or, whether time-limit or amount-limit test, the scores may be reduced to the hundredths of a second required to perform a definite minimum of work such as adding two figures, cancelling one letter, etc. Bair, in his "Practise Curve,"[47] used units both of errors made after a given number of practises, and of number of trials necessary to eliminate all errors. His curves then slope down from left to right. Bryan and Harter[48] in their study of the acquisition of telegraphy used the number of letters tapped per minute. Swift[49] in his experiments with the typewriter used the number of words written during an hour, smoothing the curve by averaging each successive three scores. In later similar work undertaken with Schuyler,[50] two units were used, one of strokes made on the typewriter, one of errors made. His curves then—for no tables are given—show one a rise, the other a slight drop. Coover and Angell[51] in making tests on the vexed question of the general practise effect of special exercise, used variously the number of right judgments before and after training, the decrease in time in 100 reactions, and the similar decrease in errors. Where practise has meant a long period of exercise taken regularly on successive days, the unit may be the average deviation of each

[47] *Mon. Suppl. to Psych. Rev.*, 1902.

[48] *Psych. Rev.*, **4**, 1897, and **6**, 1899.

[49] *Psych. Bull.*, **1**, 1904.

[50] *Psych. Bull.*, **4**, 1907.

[51] *Am. J. Psych.*, **18**, 1907.

day's performances, giving a downward sloping curve for any one individual.

So long as only one individual's curve is being considered, or only the mean curve, the use of such varied units presents little difficulty; but when comparisons are to be made of the curves of learning whether of different subjects in the same test, or those of the same subject in different tests, it becomes important to know whether a different choice of units may show the same performance in two different ways, and whether the units are alike all through the curve. Otherwise, the questions "Does practise increase or decrease differences?" and "Who profit most by practise, those whose initial record is best or poorest?" may receive quite different answers according to the varied statistical treatment of identical facts.

There is considerable divergence of custom. One method has been to keep all scores in gross amounts, basing conclusions directly on them. Examples of this would be Swift's and Schuyler's work already referred to, and Smythe Johnson's experiments on motor education.[52] Let us call this the gross method.

Another method is to turn each score into percentile values of the initial record, or perhaps of the maximum reached before fatigue sets in. Examples of this are Gilbert's work on development of school-children,[53] Oehrn's on the work-curve of 10 subjects,[54] Coover and Angell as already referred to, and Wells in reports before the New York Branch of the American Psychological Association. Let us call this the percentile method.

Another way of expressing percentile values used by Smythe Johnson,[55] and modified by him from Amberg[56] is as follows: The difference between the first and second scores, first and third, and so on, is taken, and the sum of gains so found averaged and expressed in percentage of the first score. This process is repeated with the second score used as basis, again with the third, and so on through the series. Finally, all percentages are averaged. He says "The significance of such percentages is that they give us a true standard for the comparative influence of practise on different individuals" (page 61). That part of Amberg's method which was modified was, instead of averaging the $n-1$ different percentile values, to weight each one, multiplying the first by $n-1$, the second by $n-2$, etc., adding the products and dividing by $(n-1)+$

[52] *Yale Studies*, **6**, 1898.

[53] *Yale Studies*, **2**, 1894.

[54] *Psych. Arbeiten*, **1**, 1896.

[55] *Yale Studies*, **6**, 1898.

[56] *Psych. Arb.*, **1**, 1896.

$(n-2)+(n-3)\cdots 1$. According to Amberg the resulting figure "giebt mithin in möglichst einwandfreier Weise" the average percentile increase by practise for the whole test.

Just to illustrate to what various conclusions one may be led solely from differences in methods of portraying practise data, the following tables and figures were made from five supposititious cases.

In 15 seconds, using as a score units of gross amount, suppose that in seven trials, five subjects scored as follows:

TABLE XXXVII

Gross Amounts in Successive Trials

Individual								Total Increase Units
A	5	6	7	8	9	10	10	5
B	9	12	16	16	17	17	18	9
C	10	10	10	12	13	14	15	5
D	6	9	11	12	12	15	18	12
E	5	7	9	10	12	14	15	10
Average ...	7.0	8.8	10.7	11.6	12.6	14.0	15.2	8.2
A.D.	2						2.25	

It might be stated then that D improves most, and A and C improve least.

This same table turned into units of time required to do one unit of work, using hundredths of a second as the basis becomes:

TABLE XXXVIII

Gross Time for Work Unit in Successive Trials

Individual			Hundredths of a Second					Total Decrease
A	300	250	214	187	166	150	150	150
B	166	125	93	93	88	88	83	83
C	150	150	150	125	115	107	100	50
D	250	166	136	125	125	100	83	167
E	300	214	166	150	125	107	100	200
Average ..	233	181	155	136	124	110	103	130
A.D.	60						19	

It might be stated now that E improves most and C improves least.

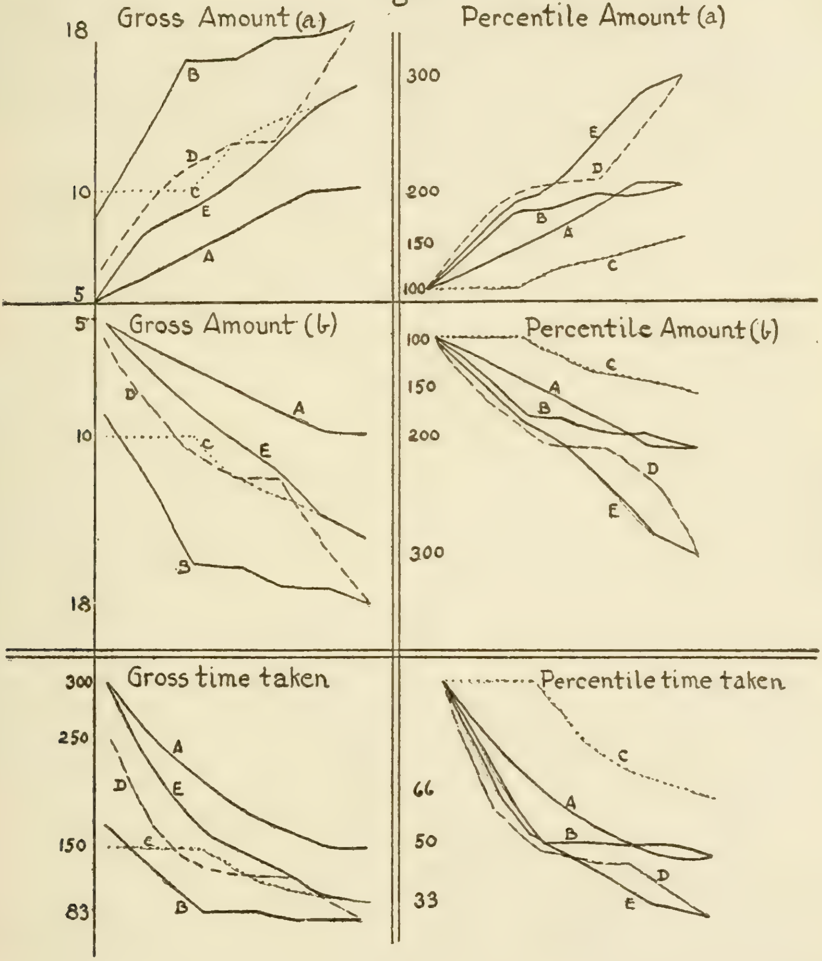
The two sets of curves as plotted* are not strictly comparable, except that the same individuals are alike at the starting point in each, and at the end. Otherwise, in answering the question whether differences are increased or diminished by practise, the curves show graphically that in the first case they apparently are increased, in the second considerably decreased. The tables show the same thing,

* See Fig. 1.

if the A.D. for the first trial is compared with the A.D. for the last, in each table. In the first case there is a slightly greater difference at the end, in the second, there is less.

The inference is then, that the change from the use of one kind of unit to another in expression of one and the same performance makes an appreciable change in its interpretation.

Fig. I.



Suppose however, as is sometimes the case, it were desirable to compare one individual quantitatively with another, it could be said from the first form of presentation that A and C improve equally, and half as much as does E; and that B improves three quarters as much as D. In the second case it might be said that no

two subjects improve equally though A and D are nearly equal; that A improves three times as much as C, and three quarters as much as E.

Evidently the value of such statements would be conditioned by the nature of the test, for units near the physiological limit would not be equal to those in the lower ranges. In a test such as mental multiplication, the gain of the last few units may be far more difficult than that of the first many. In a cancellation test, the units may possibly be of rather more equal difficulty, conditioned as they are by factors of amount of eye movement necessary, and rejection of wrong stimuli. In a feat such as juggling with balls, the first three or four units may be harder to gain than fifteen such units later. In other words, sharp slants or a plateau may be produced by the nature of the variations in the real value of the units scored as equivalent, so that a "typical" curve for certain work may really exist.

If, as is more customary when individuals are to be compared, the method of percentile values is used, the above table of gross scores becomes:

TABLE XXXIX

PERCENTILE AMOUNTS DONE

								Total Gain
A	100	120	140	160	180	200	200	100
B	100	133	177	177	188	188	200	100
C	100	100	100	120	130	140	150	50
D	100	150	183	200	200	250	300	200
E	100	140	180	200	240	280	300	200
Av.	100	129	156	171	188	212	230	130
A.D.	0	15					56	

From this it could be said that D and E improve most and C least.

Again turning this table into units of time taken and expressed in percentile values of the starting point it becomes:

TABLE XL

PERCENTILE DECREASE IN TIME TAKEN

								Total Improvement Per Cent.
A	100	83	71	62	55	50	50	50
B	100	76	56	56	53	53	50	50
C	100	100	100	83	76	71	66.6	33.3
D	100	66	54	50	50	40	33.3	66.6
E	100	71	55	50	42	36	33.3	66.6
Average	100	79	67	60	55	50	46	
A.D.	0	9.8		9.8			10.8	

As from the preceding table, the conclusion would be that A and B make equal gain, that so do D and E, and that C gains least; but whereas before C's gain was half A's and B's, and one fourth D's and E's, now it looks like one half that of D and E. Again, in each table of percentile values the A.D. tends to increase, and evidently, since in the curves the starting point is a common zero, they inevitably diverge later, and might be interpreted to mean that differences increase by practise.

In general then, this particular use of the method of percentiles must confuse the issue unless each individual's starting point is given, *i. e.,* unless some statement of gross scores is also made.

Working over the original scores given above by both Smythe Johnson's and Amberg's methods, the percentile increase is as follows:

	A	B	C	D	E
Smythe Johnson	23	19	15	38	40
Amberg	32	29	19	53	56

Here the subjects keep the same relative position, though the statements of *how much more* one improved than the other would not be alike in the two cases. E improves most and C least is all that can be said.

Just to put these varying interpretations into strong contrast the following table has been prepared, giving for six ways of expressing the facts very varying answers to the question of relative improvement.

TABLE XLI

IMPROVEMENT OF SEVENTH OVER FIRST PRACTISE PERIOD IN

Individual	Gross Amount Work Units	Gross Time per Work Unit	Percentile Amount Work Units	Percentile Time per Work Unit	By Smythe Johnson	By Amberg
A	5	150	100	50	23	32
B	9	83	100	50	19	29
C	5	50	50	33.3	15	19
D	12	167	200	66.6	38	53
E	10	200	200	66.6	40	56
Av.	8.2	130	130	53.3	27	37.8
Gained most	D	E	D E	D E	E	E
Gained equally	A C	None	D and E A and B	D and E A and B	None	None
Gained least	A C	C	C	C	C	C
Other statements	E gains twice as much as C or A	E gains four times as much as C	E gains twice as much as A and four times as much as C	E gains twice as much as C	E gains between two and three times as much as C	E gains nearly three times as much as C

One more such case will be considered, but, for brevity, instead of the similar four first tables and curves only the first and last scores in gross amount of work done by four subjects in 10 units of time is given, and a set of comparisons worked out as in the table just preceding.

TABLE XLII

IMPROVEMENT OF LAST OVER FIRST PERIOD OF PRACTISE IN

Score First	Score Last	Individual	Gross Amount Work Units	Gross Time per Work Unit	Percentile Amount Work Units	Percentile Time per Work Unit	By Smythe Johnson	By Amberg
20	30	W	10	17	50	66	14.7	17.3
12	20	X	8	33	66	40	19.2	19.6
15	25	Y	10	26	66	40	23	27.7
16	24	Z	8	21	50	33	14.5	17.0
Average			9	24.2	58	45	17.8	20.4
Most gain			W Y	X	X Y	W	Y	Y
Equal gain			W and Y X and Z	None	X and Y W and Z	X and Y	None (W and Z)	None (W and Z)
Least gain			X Z	W	W Z	Z	Z	Z
Other statements			W gains more than Z	W gains less than Z	W gains equally with Z	W gains twice as much as Z	W gains slightly more than Z	W gains slightly more than Z

The conclusion would be that if one wishes to compare one individual with another in rate of improvement, or one individual's performances in two different kinds of tests, any statement based upon a comparison of difference between the last score and the first score will be seriously affected by the kind of units chosen, and may be the more misleading the more definitely comparative they are made. All of these methods alike ignore the actual starting and finishing points which might be useful objective data, and may outrage the sense of fairness by equating units taken from different points of the scale. Thus it seems absurd to call A and C equal because each gains 5 units, since they start and finish at such different points. But to imagine that expressing A's performance as 100 per cent. gain, C's as only 50 per cent. and therefore conclude that A does twice as well as C, may be equally absurd, since it may be no nearer the truth than was the first statement. There is no magic in percentile statements, except it be in blinding people to the actual efficiency of a performance.

Then too, useful information may be obscured by stating merely the amount of *gain* or *loss* whether in gross or percentile statements, information which the full tables would have given and which is of interest; such as, in the first example, that at the start C is much better than E, but after seven periods of practise their performances

are equal, and that A after practise reaches only the point where C started. Also from the second example, W who was best at the start maintains his lead and is best at the finish; X who was poorest at the start was also poorest at the finish. Facts such as these are not brought out by a mere statement of gain, nor by the percentile tables and curves, though they would be by the gross amount tables and curves; yet they are of value in application to everyday tasks where objective norms must hold in speed and accuracy.

At this point examples may well be given of the treatment actually given to practise records—or fatigue. Gilbert[57] argues in favor of the percentile measures thus: "To have expressed the fatigue merely by the difference between the two rates of tapping would not have expressed the truth: *e. g.*, one child who tapped 19 and 15 for the respective periods of 5 seconds lost a great deal more than another who tapped 38 and 34 respectively: each lost 4 taps but the first lost 21 per cent., the second only 11 per cent." His curve shows the average per cent. of loss for each age, which means for eleven-year olds, that children whose records were 30 to 24, 35 to 28, and 25 to 20 were considered equal. Later he says, "The average boy . . . taps 29.4 times in five seconds, the average girl taps 26.9 times, thus tapping 8.5 per cent. slower than boys. The average boy . . . loses 18.1 per cent. by fatigue, the average girl loses 16.6. In other words the boys lose 1.5 per cent. more by fatigue and yet tap 8.5 per cent. faster. This leaves the balance greatly in favor of boys." Elsewhere, however, he does give a table of gross averages.

Wells, in a report read before the New York Branch of the American Psychological Association in 1910 quoted some practise results in two different tests without giving starting points, concluding that as there was 71 per cent. gain in one test and 94 per cent. in the other, there was greater gain in this than in the first. In a published article on practise in free association[58] the curves in that test are plotted on the gross decrease in units of time; but when comparison is made of susceptibility to practise in this test and in two other tests, no gross figures for the others are given at all but only the ratio of the mean of the nineteenth and twentieth days to the mean of the first and second days practise, and the conclusions based on those ratios.

Davis in his studies of cross-education[59] gives no gross gains, only the percentage. The ratio is taken on the basis of the first trial which is called 1; then the result is stated that the left hand gained

[57] *Yale Studies*, **2**, 1894.

[58] *Am. Jour. of Psych.*, **22**, 1911.

[59] *Yale Studies*, **8**, 1900.

more than the right. In earlier work[60] on the same problem he quotes initial and final scores, gross and relative gains, and plots his curves in gross errors.

Woodworth and Thorndike[61] carefully point out that one's interpretation of what equal improvement or indeed proportionate improvement means depends upon what is taken to be the starting point, and they recommend the use of at least two measures of accuracy. They use the gross error, also the ratio of errors after practise to errors before practise, so that improving from 166 to 130 errors or 78 per cent., is considered about equal to improving from 302 to 232 errors, or 77 per cent. Later a statement occurs, "the improvement in—is not equalled in the other functions." Seven years later Thorndike gives this warning:[62] "In estimating individual differences in amount of improvement . . . the ratios listed must not be taken thoughtlessly at their face value. For a person to change from 400 seconds per example to 200 is not necessarily the same amount of improvement as for him or another to change from 200 seconds to 100 seconds. The second is probably an improvement which fewer individuals would be capable of, which the same individual would take longer to attain. . . . To call the two equal as fractions must not lead one to infer any thorough-going equality in the facts which the fractions only partially represent. . . . In fact every measure of improvement by a gross difference or by a ratio must be accompanied by a statement of the initial or final gross actual ability." Such statements are given both in this and in later work,[63] where no conclusions are drawn as to whether one individual improved more or less, especially by how much more or less than another. In presenting a curve which might be representative of the general law of change, whether from the beginning of the test to the end, or between two arbitrarily chosen points each within every individual's compass, it is plotted according to the central tendency of a series of points determined for each individual by the formula

$$\frac{\text{first score—score in question}}{\text{first score—last score}}.$$

But this average or mean curve is characterized as mongrel since changes in the rate of improvement are due "to the action of radically different laws acting on different individuals according to the

[60] *Yale Studies*, **6**, 1898.

[61] *Psych. Rev.*, **8**, 1901.

[62] *Am. Journ. of Psych.*, **19**, 1908.

[63] *Am. Journ. of Psych.*, **21**, 1910.

different physiological changes in them to which the improvement is due.''

It would seem then that the answer to the question ''How much relative improvement is there, or how much more does one individual improve than another?'' can be given only for some arbitrarily chosen definitions of ''how much'' and ''how much more.'' The nature of the work, the inevitable relativity of the starting points and of the units, and one's preferred method of interpreting statistics will all modify such answer. What must be done is to keep the first factor in mind, to present the second fully, and in more than one way, to be wary and undogmatic as to the third, allowing others to be the same.

There are other questions commonly asked, however, and answered simply from examination of curves plotted according to gross amounts, or somewhat variously by the use of certain formulæ.

For example, it is of great importance in relation to measurements of the relative parts played by heredity and environment in producing the differences between individuals to determine whether, and how far, different amounts of training account for individual differences. The most usual and convenient measurement is of whether and how far equal amounts of practise will reduce individual differences. To make this measurement one might:

1. Examine the average deviations from the average at the first trial, and also after practise, and compare them directly. Then according as one's units of measurement increase in amount or decrease in time or error, so will the deviations in all probability.

2. Use the formula $\frac{\text{A.D.}}{\text{Av.}}$ for both beginning and end, and make comparisons.

3. Use the preferred formula $\frac{\text{A.D.}}{\sqrt{\text{Av.}}}$ and compare.

4. Study the ratio of the range at both the beginning and at the end, by finding in each case the ratio of best to worst, second best to second worst and so on, and comparing each such ratio with the corresponding ratio at the end.

Moreover any of these four methods could be applied not only to the first and last scores, but to averages of the first few and the last few, or the middle, or to each if necessary. Using all four methods on the two examples given, the figures would stand:

TABLE XLIII

FROM EXAMPLE 1

	Gross Amount		Gross Time		Per Cent. Amount			Per Cent. Time		
	First	Last	First	Last	First	Second	Last	First	Second	Last
Average	7	15.2	233	103	100		230	100		46.6
Gross A. D.	2	2.24	60	18.6	0	15	56	0	9.8	10.5
A. D./Av.	29%	14%	25%	18%	0	11%	24%	0	12%	23%
A. D./$\sqrt{\text{Av.}}$	75%	57%	393%	183%	0	132%	369%	0	110%	154%
Worst and Best .	2.00	1.80	.50	.55	0	1.50	2.00	0	.66	.50
Next Worst / Next Best	1.80	1.20	.55	.83	0	1.16	1.50	0	.85	.66
	or from twice as good to 1.80 times as good		or from half as good to .55 as good		or from 1.50 as good at the second trial to twice as good			or from .66 times as good at the second trial to only half as good		

TABLE XLIV

FROM EXAMPLE 2

	Gross Amount		Gross Time		Per Cent. Amount		Per Cent. Time	
	First	Last	First	Last	First	Last	First	Last
Average	15.7	24.7	65	41	100	139	100	63.3
A. D.	2.2	2.7	9.2	4.5	0	1.9	0	3.3
A. D./Av.	14%	10%	14%	11%	0	13%	0	5%
A. D./$\sqrt{\text{Av.}}$	57%	54%	114%	70%	0	161%	0	42%
Worst and Best ..	1.66	1.50	.60	.66	0	1.16	0	.91
Next Worst / Next Best	1.06	1.04	.94	.95	0	1.16	0	.91
	or from 1.66 times as good to 1.50 times as good		from .60 as good to .66 as good					

From the tables in gross amounts it would be concluded that individual differences tend to increase with practise; but the terms in which the score is kept, and the method of comparing variations make a great difference in the apparent amount or ratio of that decrease. The last method illustrated needs perhaps a word of caution. In the second column—although the figures increase from .50 to .55 and .60 to .66, this means a decrease in differences of range, as the interpretative readings added for both the first and second columns show. Obviously, in the next two columns by the percentage increase or decrease scoring, individual differences must be shown to increase by practise, since all are made to start equal. The answers to the questions obtained by such methods are then necessarily absurd.

Therefore in using any of these four methods to examine the variability one should again: (1) beware of being misled by the kind

of units used, both at the chosen starting point and at any point in the practise series: (2) prefer gross to percentile measures of the ability in question: (3) remember that only general tendencies are given, not specific comparisons.

Even the fourth method would not make comparisons always between the same pairs of individuals unless they happened to retain their relative position all through the series, since it is engaged in studying the range whoever may be at or near the extremes. But this very point of individual comparisons is also of interest—whether the one who is best at the start is also best after practise even though the curve may have a less sudden slant than that of the worst at the start, and whether those who start with a poor record will still be poor, or the poorest at the end. The fourth method could be modified to answer that, but there are at least two common procedures. One is to compare the position at the start with the total gross gain or percentile gain or both; the other is to rank all individuals at their first trial and at their last trial and compare the rankings.

By the former method, applied to example 1, between ability at the start and gross gain there is correlation of —.32; between ability at the start and percentile gain a correlation of —.55, from which the inference would be that those who start well gain less than those who start poorly.

By the latter method (used by Wimms[64] in his work with schoolboys in various mental tests) correlating by the "foot-rule" method, $R = .75$.

Even this ranking method has been variously applied. Wimms, for instance, also tabulates the percentage increase of each of his subjects from the first to the last series of tests and ranks his subjects accordingly. He then finds that the two ways of ranking, this, and by numerical difference of absolute achievement in the last series, do not agree.

Oehrn,[65] whom Wimms quotes, after stating that practise has two effects, that of shortening the time for successive groups of trials, and that of reducing each subject's variability in series of such groups, ranked his subjects first in decrease in gross time taken, also in percentage of reduction of variability, and found that the two ways of ranking were not proportional. His correlations are based on the ranking for the time taken. In his work too he introduces another point as the basis of reckoning for the "work-curve," namely the maximum performance of any individual, which he says is a better standard than the starting-point because more constant

[64] *Brit. Journ. of Psych.*, **2**, 1907.

[65] *Psych. Arbeiten*, **1**, 1896.

for each individual. This is rather a novel procedure, which though it may have suited his conditions—continuous mental work for two hours measured every quarter of an hour—would not suit work like Bair's or Bryan and Harter's where the maximum performance was emphatically not a constant.

In general, this ranking method tells precisely what a direct inspection of individual curves would do; but since with large groups it would be inconvenient and confusing to plot all the curves, tables of ranks would be likely to give direct information about relative improvement. If the question were "Are those who are best at the start also best at the finish?" then ranks in initial and final tests would be needed. If the question were "Do those who are best at first improve most or those who are poorest?" then ranks by the initial record and total increase would be needed. The absolute gain would be the more objective record perhaps, but here, at least, so long as gross measures are available, a percentile or proportional gain would not be misleading, and would often give just the practical information required.

Now this tedious elaboration has been based on simple and supposititious records, solely to bring out possible discrepancies in results and conclusions according to the use of one method rather than another. Actual published results could be worked out in the same way and contrasts drawn. That would, however, be beyond the scope of the present investigation.

That the practise or rather the "work-curve" may be complicated beyond easy and rapid inspection, Kraepelin has endeavored to show[66] when he takes the record of one subject in continuous work for two hours and at great length analyzes and plots curves for at least seven factors: practise, fatigue, adaptation (or warming-up period), inclination (or attitude towards work), initial and final spurts, the desire to improve, and recovery by rest. He points out, too, the difference between morning and evening workers, and the effects of a recent meal or period of sleep.

Who would study individual differences as revealed in or affected by practise has no easy task.

2. Results from a Special Series of Tests

So far in this study, the statistics of practise with the short or long term groups have been confined to the starting point, average and finishing point in gross amount for each group, with no comparison of individuals. Too few subjects made up the long-term group to make any extended comparisons worth while, and the larger

[66] *Phil. Studien,* **19**, 1902.

group made too few trials with most tests to do more than indicate the trend of individual curves at the beginning of practise.

Also, the results have been stated as if a typical curve for a test or a group of tests could be determined. But it is a question whether individuals will not differ so much in their improvement with any test as to make the average or mean curve unreliable, or rather representative of nothing. It is also a question whether an individual's improvement in one test will not so parallel his improvement in another as to make his curve typical of him rather than of the kind of work. Or again, a "motor minded" individual might show a different rate of practise in a motor test from one who is an abstract thinker, and different also from his own improvement in another field. In other words is "the practise curve" that of (1) the kind of work, or (2) of the general abilities of an individual, or (3) of special abilities of individuals?

In the hope of getting a little light on this problem, a further set of tests was undertaken with a larger group of subjects, a long period of practise, and with five tests of presumably very different functions.

Supposing tests could be selected with which the subjects had had no previous experience, then if all show slants and plateaus at about the same level of practise judged by time or amount, the curve would be typical of the kind of work. If there is greater resemblance between all curves from one individual than between one individual and other, then the curve is typical of the kind of person rather than of the kind of work. If any one subject's curves in, say two motor, or two mental tests, resembled each other and were unlike the mean curve, but in tests of some other function were like some other individual's curves, then the curve is typical of specialized abilities in individuals. Lastly, if the mean curve for one individual in several tests is indistinguishable from the mean curve of several individuals in one test there would be no evidence one way or the other except that practise must produce the same results in people whatever the work, and so must reduce differences between people.

In order to discover which of the above conditions would prevail, a group of subjects was put through a period of practise for twenty days, excluding Sundays, in November and December of 1909.

The subjects, nine in number (the tenth did not continue sufficiently long for any use to be made of her records) were all women selected from among Teachers College students on the basis of their needing financial help in working through college and so responding to an appeal for subjects. From the group those were used who

could give from one and one half hours a day at the beginning to whatever time the tests took at the end of the period of practise, always at the same time of day. Four distinctly different nationalities were represented, and five different departments in the college. One was constitutionally delicate, two others showed signs of strain and worry, the other six were in good health. One was over forty, one over thirty, the others under twenty-five. Their college standing for the year 1909–10 was also examined, and they themselves were carefully observed for general temperament as revealed during the practise of one test. These facts are tabulated below:

Subject	Nationality	Department	Health	Relative Age	College Standing
C.	American	Mathematics	Delicate	Young	Good
E.	American	Eng. & Dom. Sci.	Tired	Over 30	Very good
Go.	Russian Jewess	(German)	Good	Young	Variable
H.	American	English	Good	Young	Poor
Jb.	German	Domestic Art	Good	Young	Fair
Nb.	American	English	Good	Young	Good
P.	American	English	Good	Young	Fair
Sch.	German	German	Good	Over 40	Good
Sa.	Jewess	Physical education	Strained	Young	Fair to good

The tests selected were five in number: one for accuracy and speed in movement, one for sensory discrimination, one for discrimination plus movements, one cancellation or perception test, one purely mental test. The tests were explained orally to the subjects and demonstrated, after which a manuscript book was given to each with the directions for each test written out, and spaces prepared for the required entries. The subjects were asked to select whatever time of day was most convenient for them, and to work always at that time through the whole number of days that the tests lasted. Four of the tests were thus practised independently and always in the same order; but for the discrimination of lifted weights, which test needs of course an observer, each subject came at an appointed hour.

For the first test the *curved maze* already described (see page 87) was used. The directions were as follows:

"1. Place the maze so that the words *begin here* are at the left-hand bottom corner. Do not turn the paper about during the test. See that you have a sharp pencil.

"2. Note the time when you begin: (wait until the second hand of your watch is at 60).

"3. Draw a line between the two lines of the maze without touching either, working as fast as you can.

"4. Note the exact time at which you finish, entering both times in the proper columns opposite.

"5. Write your name on the blank, also the number of the experiment."

The spaces ruled for entry were headed:

Date	Time of Day	Physical Condition	Time at Start	Time at Finish

In this third column they were directed to grade their felt condition from A, excellent, to D, miserable. Thus a check of health and weather could be applied to each subject's performances.

The "purely mental" test consisted of three sums in mental multiplication of a three-place number by a three-place number. The directions were:

"1. Beginning at the middle of this book you will find, under *day 1, 2,* etc., three sums to be multiplied, each 3 figures by 3 figures.

"2. Cover up all but the one to be worked; take note of the time.

"3. Multiply it mentally. Do not write anything at all till you get the final answer, then write that down.

"4. Record for each sum in the appropriate column the time at the beginning and the time at the end. Do not rest more than three minutes between examples."

This wording might have been still more explicit, but the subjects understood that "take note of the time" meant to write it down, and also that the recording was to be for each sum, not after all three were finished. The spaces for entry were headed:

	First Sum		Second Sum		Third Sum	
	Time at Start	Finish	Time at Start	Finish	Time at Start	Finish
Day 1.						
Day 2.						
Etc.						

For the sorting test, Dennison's colored cardboard counters 1½ inches in diameter, $\frac{1}{20}$ of an inch thick were used, and for the "box," the 5-cent size ice-cream carton. The directions were:

"1. In the little bag are 50 counters all of one color; in the box are 50 counters of five different colors. Empty the varied ones into some convenient place, and empty the bagful into the box.

"2. Distribute the 50 from the box at random into five piles. In doing this use one hand only, and pick up only one at a time. Work as rapidly as possible. Do this twice, just for practise in manipulating the counters. Return them to the bag.

"3. Shuffle the 50 mixed colors well, and put them into the box. Time yourself as in the other tests, and sort the 50 into five heaps *according to color,* using the same care in handling as before. Record the time at the finish.

"4. On the 1st, 10th, and 20th days, record also the time before and after *one* distribution of the 50 all of one color."

Spaces were prepared for the entries of time at start and finish each day as before, also for the three additional entries.

For the cancellation test, two copies of each of two back numbers of the *Journal of Philosophy, Psychology, and Scientific Methods* were provided for each subject. From these certain pages were selected which were fairly evenly filled with print, in the hope of getting about the same number of *a's* for each experiment, also about the same number of lines for the eye to traverse. Previous work with this test had shown how soon a blank is memorized, so that it seemed advisable to use more ordinarily available reading matter. Pages of a foreign text would have been still preferable.

The directions were:

"1. Find the pages for the day: be ready to turn over quickly. Note the time.

"2. Mark, on the pages designated every small print *a* you see, going line by line over the two pages. To underline is the quickest method.

"3. Note the time at start and finish as before."

The spaces for entry were headed as before, besides indicating for each day exactly which pages were to be used. A second trial with the same page was made only four times, and then it came at least ten days later than the first trial, so that there was practically no memory of the location of the *a's*. The average total number of *a's* for the daily task was found to be 338, but unfortunately with a large range of from 268 to 410, which complicated the latter calculations very much.

For the *lifted weights test* thirty weights ranging from 40 to 130 grams were prepared. These were unpainted wooden cylindrical boxes containing lead or small shot to make up the required weight. Six of these were used as standards of comparison, a 40, 55, 75, 90, 110, and 130 box, so labelled, and kept apart by themselves to the side of the twenty-four test boxes. Of these, there were nineteen different weights ranging by differences of 5 grams from 40 to 130 grams, and also six duplicates, one each of the 45, 60, 75, 90, 105, and 120 gram weights. It will be noticed that of these duplicates two are identical with two of the standards. By using six standards scattered through the range, and by using steps of five grams it was hoped to make the test easier and therefore likely to be completed more rapidly than if merely one of the extremes had been used as

the sole standard or if very fine discriminations had been necessary (see Thompson's work[67]).

The twenty-four test weights were arranged in three rows of eight, and daily rearranged in a different order with care to avoid strong contrast effects and consequent probable illusions. Secret marks on the side nearest the observer permitted immediate and rapid checking up of the judgments made. For the first two days preliminary experience was allowed in hefting the six standard weights and one or two test weights. Thereafter the subjects began immediately upon the test.

The first box in the nearest row was hefted with the fingers of the right hand, then one of the standards, whichever would be selected as probably the nearest, then the judgment was generally made in terms of grams. However the subjects were free to try another standard if the first was presumably not near the testbox in weight and then to heft the testbox again. In this way emphasis and help were given to making correct judgments. No fixed speed was insisted on, but a check was kept on the total time taken daily for the whole set of twenty-four judgments. Only on three occasions were subjects hurried up, and then when they had exceeded 25 seconds in arriving at a judgment. Otherwise the aim was to leave the subjects as free as possible.

Each subject came 16 times for this test, though as all did not begin on the same day, any particular arrangement of the boxes would not fall on say the fifth trial for everybody. After a certain date too, each subject after having made a judgment was told what the real weight was, in the hope of facilitating practise by this means. Again, this additional means of training did not begin at the same point in the series of 16 tests for each subject. In the curves this point is indicated for each individual by a small cross.

In working up the results, judgments for weights below 60 g. and over 105 g. were not used, in order to avoid the influence of the "end error." The curves then are plotted from the average error in 14 judgments of 10 different weights from the middle of the series, 4 of which were duplicates and 2 of those duplicates identical with 2 of the standards. This leaves a total of 2,016 judgments instead of 3,024.

The method of scoring was to enter immediately the errors in grams, plus or minus. After the date on which the subjects were told the real weights, the last 12 judgments of the 24 were recorded in ink instead of pencil. In this way could be found (1) the average error with each weight for each subject, (2) the constant error for

[67] "The Mental Traits of Sex."

TABLE XLV

AVERAGE AND CONSTANT ERRORS. WEIGHTS TEST

Day	Subject S.		Nb.		Go.		P.		C.		Sch.		J.		H.		E.	
1	+5.7	10.0	+ 2.5	13.2	—11.8	19.6	— 3.2	8.9	—4.6	6.8	+ 2.5	9.6	+5.3	10.3	+5.3	9.6	—6.8	12.1
2	0	6.4	+11.1	11.8	0	12.1	+10.3	10.3	+1.1	10.0	+ 8.2	11.1	+5.7	10.7	+9.2	10.4	+2.1	7.1
3	— .3	5.7	+ 3.9	6.8	— 2.5	10.3	+ 2.9	6.4	—2.5	8.2	+ 8.6	10.7	+2.1	5.7	+3.9	9.6	—1.4	5.0
4	—1.4	3.6	+ 5.0	7.9	— 5.3	12.8	+ 1.8	6.1	— .3	8.2	+13.5	15.0	+3.5	7.1	+3.2	7.5	+2.8	11.4
5	+*2.1*	*3.6*	+ 2.8	5.7	+ 8.6	11.8	+ 2.1	9.3	+2.1	10.7	+ 5.7	8.6	—2.5	9.3	+3.6	9.3	0	9.2
6	+2.5	5.0	+ 4.3	6.4	+14.6	14.6	+ 8.9	9.6	+7.9	9.3	+ 9.3	11.3	— .3	10.7	+1.6	10.4	+5.3	8.5
7	+1.4	4.3	+ 2.1	5.0	+10.7	15.7	+ 2.1	4.3	+3.2	7.7	+ 4.3	5.7	+ *.3*	*6.8*	+6.4	8.9	+7.5	13.9
8	+2.5	3.9	+ 3.9	4.6	+10.7	14.3	+ 2.1	9.6	+3.6	6.1	+ *2.5*	*7.5*	—3.2	3.9	+3.6	7.1	+5.3	11.0
9	+2.5	5.3	— *1.0*	*6.1*	+ *5.7*	*10.3*	+ *6.4*	*8.9*	+*2.9*	*8.2*	+ .3	5.0	—1.1	5.3	+*6.4*	*7.1*	+*6.4*	*10.0*
10	+3.6	5.0	+ 3.9	7.5	+ 1.4	10.7	+ 4.6	4.6	+ .3	7.5	+ .7	7.1	+3.9	5.7	+7.8	7.5	—1.8	5.3
11	+2.1	4.3	+ 5.0	6.4	— 1.8	11.1	+ 2.9	6.1	+7.5	8.9	+ 2.8	5.0	+2.1	6.4	+8.9	8.9	+1.1	5.7
12	+3.2	4.6	+ 6.1	8.2	+ 3.2	8.2	+ 7.1	8.2	+5.0	7.9	+ 2.8	5.3	+4.3	5.7	+5.7	6.4	+3.5	7.8
13	+3.2	3.2	+ 2.8	5.7	+ 1.8	6.1	+ .3	2.5	0	3.9	+ 4.6	7.9	+3.2	7.5	+2.8	4.3	+6.1	9.3
14	+3.9	6.1	+ 2.8	5.7	+ .7	7.8	+ 4.6	6.8	+5.3	6.1	+ 3.9	9.6	+ .7	5.7	+3.6	4.3	— .7	4.3
15	+4.3	5.7	+ 4.3	5.0	+ 3.2	6.1	+ 1.1	2.5	+5.3	5.3	+ 2.1	7.1	+4.3	5.7	+2.5	4.6	—1.4	5.7
16	+3.2	4.6	+ 2.8	5.0	+ 1.1	10.3	+ 1.8	8.1	+2.1	5.7	+ 1.4	4.3	+5.0	6.1	+1.8	5.4	+2.5	7.1

each weight, (3) the average error daily, (4) the constant error daily, (5) the improvement daily during the test.

Below is given the average error for each weight through the whole period of practise:

40	45	50	55	60	65	70	75	80	85	90	95	100	105	110	115	120	125
2.1	4.4	5.7	6.1	6.3	7.8	7.2	7.1	9.1	7.8	7.6	8.3	8.2	7.9	7.3	5.5	5.3	3.9

From this the influence of the "end error" is clearly visible, though not so far into the series as it had been expected. The weights of 75 and 90 grams do seem to show the benefit of both their identity with the standards and the double practise they received; the 60 grams perhaps shows the double practice benefit, but the same can not be said of the other weight, the 105.

Table XLV gives for each subject for each day the average error and the constant error for the set of 14 judgments. The scores in italics show the first day on which additional help was given by being told the real weight.

In general this shows a slow reduction in the average error for each subject, a tendency to a positive constant error, a disturbance in the constant error on the day of the change in method, and that the greatest fluctuations occurred between the first and second trials.

Eight other individuals also took this test in this form once each. For these control cases are here given also the constant error and the average error for the set of 14 judgments.

TABLE XLVI

Ind.	Const. Error	Av. Error	Rank for Accuracy. 1 = Least Error	Rank for Time. 1 = Least Time
1	+4.3	6.4	4	2
2	+ .7	17.2	8	8
3	+2.1	5.0	1	6
4	—1.4	10.7	7	1
5	— .3	9.7	6	5
6	—1.0	6.1	3	7
7	—1.8	5.4	2	4
8	+9.6	9.6	5	3
	Average................	8.8		

Compared with the first trial by the practising group, 11.1, the average record for these is somewhat better. The curves as plotted for each of the group of nine subjects from their daily average error are shown in Fig. 2. The dotted line shows the most probable "smoothed" curve. The two individuals most unlike are Go. and Sa. The latter had the benefit of knowledge of the correct weight longer than did the others; she was also the slowest of the nine. Go. gave

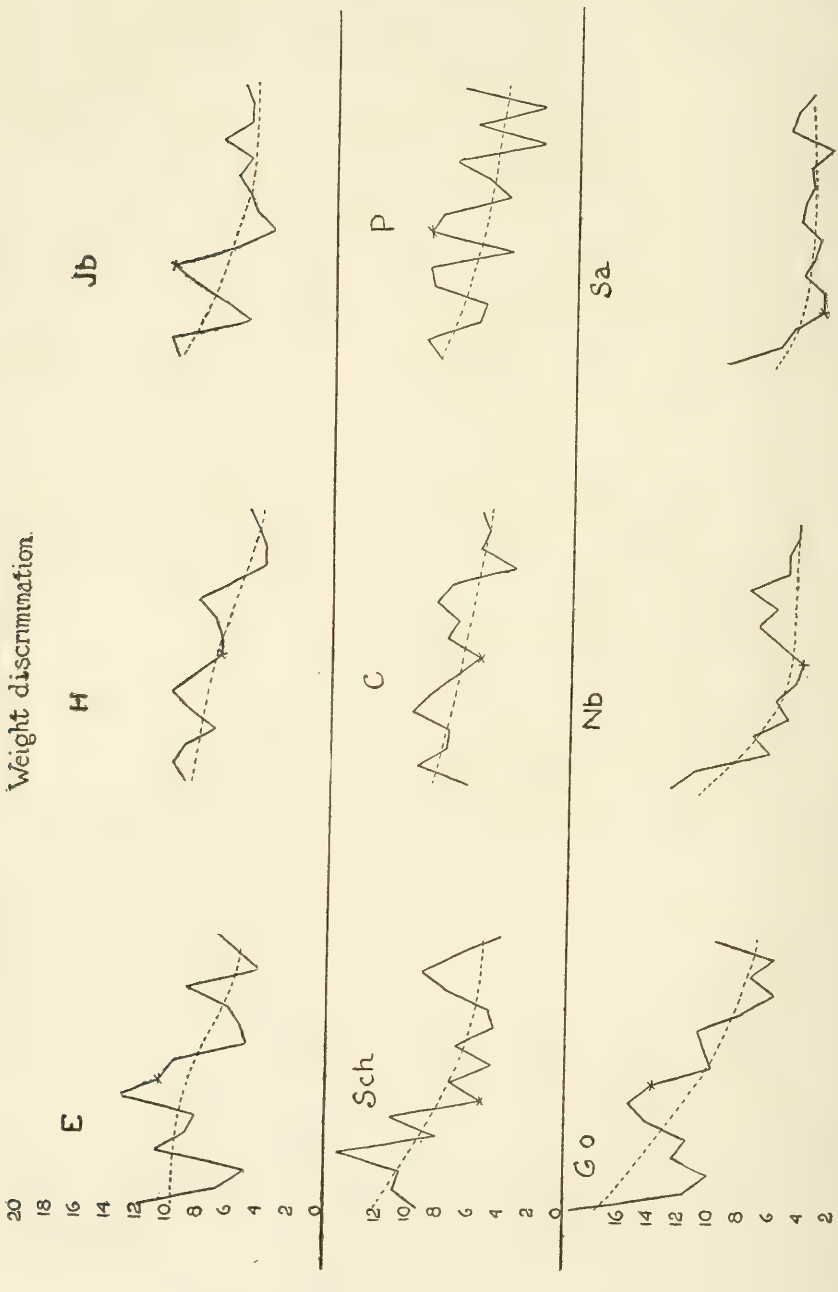


Fig. 2.
Weight discrimination.

the impression of being very careless and indifferent, she took about half the time that Sa. did. Nb., who took about three sevenths the time Sa. did and was the quickest, has a curve more like Sa.'s than has any one else. Taking the average of the first two trials and of the last two (gross score), the gross gain, percentile gain, the time taken on the average, and ranking the nine subjects by each of these scores, we get:

TABLE XLVII

	Rank at Start. (1 = Least Error)	Rank at Finish. (1 = Least Error)	Rank for Av. of Total Series (1 = Least Error)	Gross Gain. (1 = Most Gain)	Percentile Gain. (1 = Most Gain)	Time Taken. (1 = Least Time)
E.	3.5	8	8	7	9	7
H.	5	1.5	6	3	2	4.5
Jb.	7	7	3.5	4.5	6	2.5
Sch.	6	6	7	4.5	5	8
C.	2	5	5	9	8	6
P.	3.5	4	2	6	4	4.5
Go.	9	9	9	1	3	2.5
Nb.	8	1.5	3.5	2	1	1
Sa.	1	3	1	8	7	9

from which the correlations by the method of rank differences are as follows:

Position at start and at finish	$R = +.27$
Position at start and average in the whole series........	$+.45$
Position at start and gross gain......................	$-.98$
Position at start and percentile gain..................	$-.65$
Average in whole series and time taken................	$-.04$

This means that, with these subjects at least, their performance at the first two days' trial was relatively more like their average performance than it was like their performance during the last two days. Those who were poorer at the start made a greater relative gain and a much greater gross gain than those who were better at the start. Within the range of accuracy attained there was practically no relationship to the speed of judgments.

For the control cases also the correlation of accuracy and speed in this was — .07, very near the figure for the practising group, and meaning again practically no relationship.

To notice the improvement if any during the daily test the average errors of the first twelve and the last twelve judgments of each subject were compared. The twelve were of course carefully distributed over the whole range of weights. The errors are as follows:

	First 12	Last 12
E.	6.7	6.8
H.	5.1	5.5
Jb.	5.4	5.9
Sch.	5.5	5.1
C.	5.1	5.3
P.	5.1	5.4
Go.	8.2	6.5
Nb.	5.4	5.7
Sa.	4.1	5.0
Average	5.6	5.7

There is no "warming up" effect discoverable from the first half to the second half of the test daily. On the whole there is scarcely any difference, though for some subjects there is a decided increase in error, which in Sa.'s case may be due to fatigue, since she was the slowest.

The scoring of the *a's* test was not so easy, because of unequal numbers of *a's* in the daily tasks of two pages each. Instead of retaining the gross time taken to cover two pages, it seemed fairer to make the following reduction: find the time that would have been required (proportionately) to cover a space including 250 *a's* with the same accuracy as was actually shown for the whole two pages, *i. e.,* with the same proportion of errors and omissions. This reduction is accomplished by use of the formula,

$$\frac{\text{time taken}}{\text{number marked}} \times 250.$$

Thus, the score for a subject who in 420 seconds marked 286 *a's* is

$$\frac{420}{286} \times 250 = 367.$$

This score is, essentially, the time for covering a given space, and therefore grows smaller with increase in efficiency.

In the following table are given the daily scores for each individual, and also the total number of *a's* in the day's task. The curves as plotted from these scores are shown in Fig. 3. The greatest difference is from the first to the second day's trial.

There are several curves fairly similar, Jb.'s and P.'s, for instance, also H.'s and Go.'s, perhaps E.'s and Nb.'s. When smoothed out, there are seven very similar, namely those of all except H. and Go. The two most unlike are Go.'s and C.'s, the former irregular, showing a poor record at the start and a rapid improvement, the latter very smooth, with a good record at the start and gradual but steady improvement. In percentile improvement the two were nearly equal.

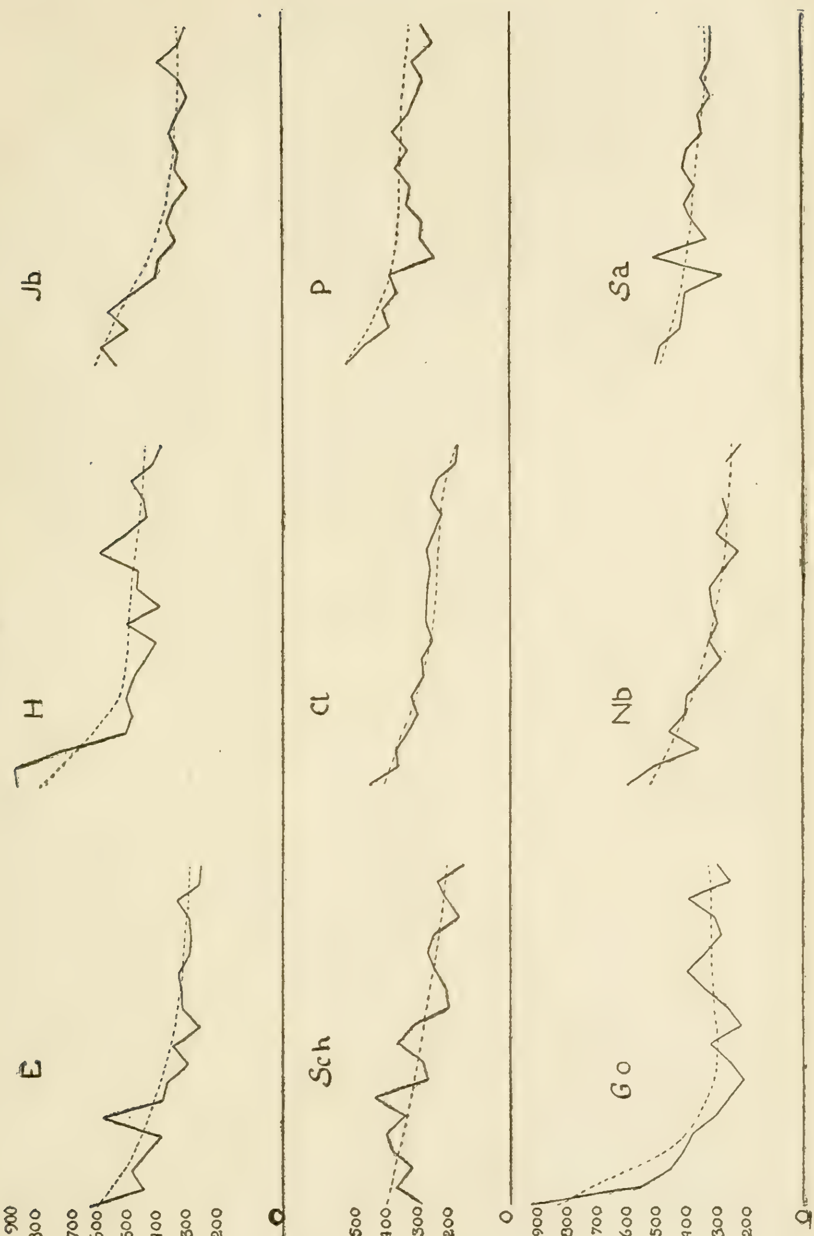


Fig. 3
Marking a's

TABLE XLVIII

SCORE IN A'S TEST

Day	a's Possible	E.	H.	Jb.	Sch.	C.	P.	Go.	Nb.	Sa.	Av.
1	299	642	882	550	298	463	542	933	611	508	603
2	314	461	892	596	374	367	480	558	517	498	527
3	299	506	737	514	330	371	398	457	372	428	457
4	285	447	508	581	392	341	418	425	476	426	446
5	345	402	502	497	413	314	366	390	418	414	413
6	374	595	520	418	352	326	391	312	416	298	403
7	355	395	496	408	442	287	257	273	348	519	381
8	368	388	458	347	282	296	302	219	299	362	328
9	365	307	421	381	293	258	293	273	342	386	328
10	318	365	517	360	373	285	341	334	310	417	367
11	409	270	409	314	323	285	329	232	324	382	318
12	333	331	484	347	209	280	383	281	335	420	341
13	327	334	481	340	210	266	343	349	285	412	335
14	268	345	611	365	251	278	390	413	242	356	361
15	315	306	520	340	277	252	343	350	315	365	341
16	360	299	453	315	262	229	312	303	275	327	308
17	334	304	465	325	170	267	297	326	294	360	312
18	313	349	493	413	209	248	334	402	?	336	348
19	410	279	431	347	246	179	262	271	279	327	291
20	409	274	400	319	146	173	304	307	236	333	277
Average	**338**	380	534	354	293	288	354	370	352	394	

As before, comparing the average of the first two days with the average of the last two days, taking also the average for the whole 20 days, the gross gain, the percentile gain, and ranking the nine subjects for each of these and also for speed and for accuracy, we get:

TABLE XLIX

	Position at Start 1=Shortest Time	At Finish 1=Shortest Time	Average Position 1=Shortest Time	Gross Gain 1=Most	Per Cent. Gain 1=Most	Speed 1=Least Time	Accuracy 1=Fewest Errors
E.	5	4	7	4	5	1	7
H.	9	9	9	1	4	9	4
Jb.	7	8	4.5	5	7.5	7	9
Sch.	1	2	2	9	7.5	3	6
C.	2	1	1	6	2	2	5
P.	4	5	4.5	7	6	6	1
Go.	8	6	6	2	1	4	8
Nb.	6	3	3	3	3	5	3
Sa.	3	7	8	8	9	8	2

The correlations by the method of rank differences are:

Position at start and at finish.......................... $R = .72$
Position at start and average position................. .58
Position at start and gross gain .... —.90
Position at start and percentile gain.................. —.38
Speed and accuracy.................................. —.37

Here the subjects kept their relative positions through the test fairly well. Those who were poorest at the start made a greater relative gain than those who were better, and had almost a guarantee that they would make a greater gross gain. The quicker ones are rather less accurate than the slower ones.

In the *sorting test* one subject so misunderstood directions that all her records had to be discarded; another so confused her first nine entries that they too could not be used; a third showed a carelessness in entering whole minutes rather than seconds. Only seven and a half complete records were therefore available of which one is not so reliable as are the others.

The following table gives the daily scores, from which the curves as plotted are shown in Fig. 4. The scores on the three occasions on which the "control time for movements only" was recorded are indicated on each curve by a cross. The missing curves are suggested by a dotted line.

TABLE L

SECONDS REQUIRED TO SORT 60 COUNTERS

Day	E.	Jb.	Sch.	C.	P.	Go.	Nb.	Sa.	Average
1	240	90		150	120	240	150	145	162
2	120	90		60	60	180	90	85	98
3	60	100		75	89	180	60	90	93
4	60	85		55	90	240	60	80	96
5	60	170		56	90	240	90	72	111
6	90	90		65	95	240	90	68	105
7	90	80		55	65	120	90	114	88
8	90	70		50	70	120	72	89	80
9	60	110		50	50	120	72	75	77
10	120	105	120	50	55	150	54	77	91
11	60	80	60	56	60	120	66	90	74
12	60	60	100	53	68	120	66	95	78
13	90	60	85	50	60	60	60	69	67
14	60	110	60	40	50	60	85	100	71
15	45	50	90	45	60	60	60	69	80
16	60	50	115	45	55	60	50	60	62
17	60	108	60	45	60	60	60	62	64
18	60	60	90	45	61	60	60	68	63
19	60	60	60	50	60	60	65	66	60
20	60	60	90	43	50	60	50	65	60
Average ...	80	84	85	57	68	127	72.5	82	

The curves most alike are those of P. and Nb., though when smoothed out those of Sa. and C. are also similar. Those most unlike are Go.—irregular and rapidly improving—and C., very regular with almost all the improvement at the beginning. Since Go.'s scores were so poorly kept, a better instance of dissimilarity might be C.'s

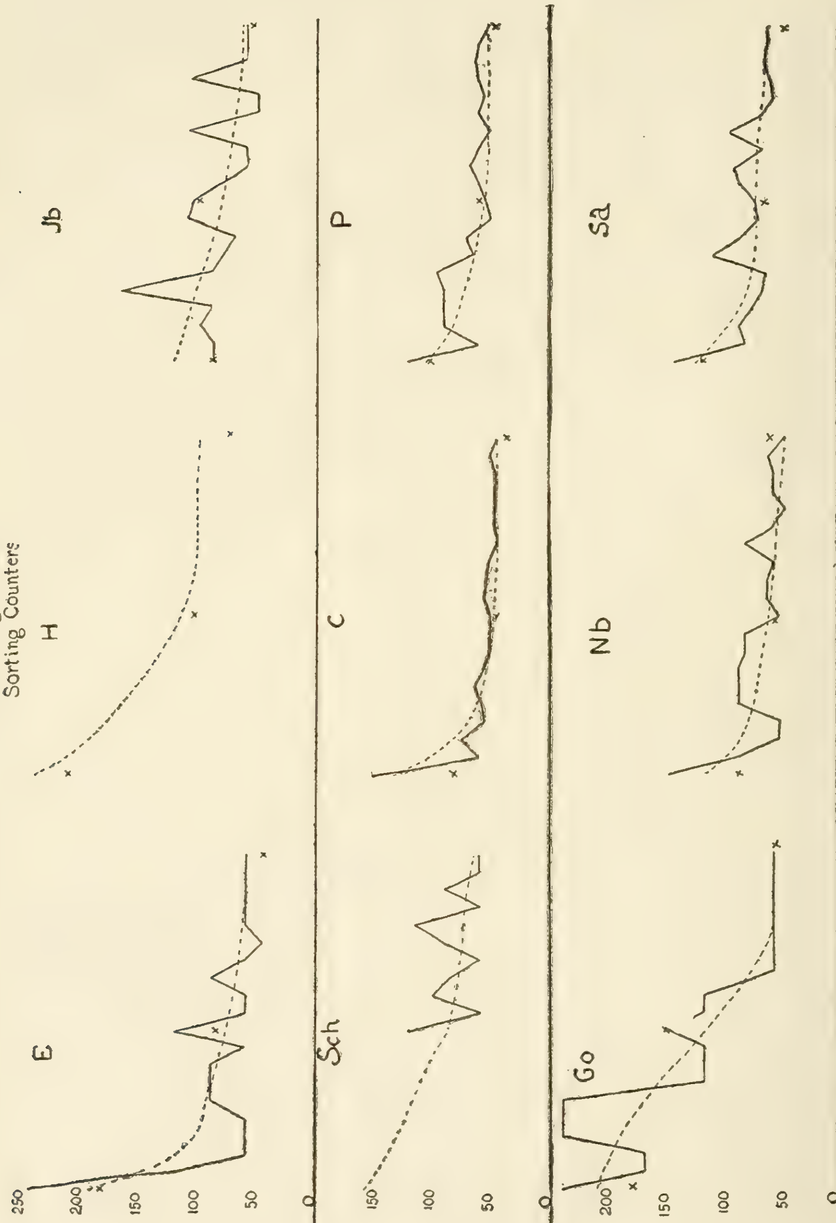


Fig 4
Sorting Counters

curve and Jb.'s, the latter showing great irregularity from day to day and the reverse of improvement near the beginning.

Below are the rankings of the subjects according to position at the start (average of two days), position at the finish, average position, gross gain and percentile gain.

TABLE LI

	Position at Start 1=Least Time	At Finish 1=Least Time	Average 1=Least Time	Gross Gain 1=Most Gain	Per Cent. Gain 1=Most Gain
E.	6	4.5	4	2	2
Jb.	1.5	4.5	6	7	7
Sch.	—	8	(7)	?	?
C.	3	1	1	4	3
P.	1.5	2	2	6	6
Go.	7	4.5	8	1	1
Nb.	5	4.5	3	3	4
Sa.	4	7	5	5	5

From these the correlations by the method of rank differences are:

Position at start and at finish	$R = .56$
Position at start and average position	.58
Position at start and gross gain	—.92
Position at start and percentile gain	—.86

Here there was more change in the relative position through the test than in the marking *a's*. It should be noted, however, that the "positions" were very close together at the end since nearly all got down to about 60 seconds or slightly less in handling the 60 counters.

Again therefore, since all finish nearly alike, those who were poorest at the beginning made the greatest relative and gross gain.

In the *mental multiplication* tests only digits from 3 to 8 were used in the multiplicand, and from 2 to 7 in the multiplier. In arranging examples care was taken to have no two consecutive figures alike in both multiplicand and multiplier—to minimize unnecessary confusion. The subjects all dreaded this test at the outset, but after two days' work with it they gained confidence in their ability. No suggestion was given any of them as to using or discarding visual or auditory imagery, nor as to devices for lessening the number of figures to be remembered. But they were asked to note any change in attitude or method that helped or hindered them. The following notes are interesting.

E. after the second day decided that a pause between examples was not worth while. For a time she visualized a series of dots as a help in placing partial products.

H. found it better to do her adding as she went along rather than to keep one partial product in mind while getting another.

Jb. discarded visualizing as it was a hindrance. She tried saying the partial products aloud for awhile, finally took to adding two partial products before finding the third.

P. also hit upon this method as early as the third day and kept to it thereafter.

C. occasionally adopted a device, such as, with a multiplier like 625, dividing by 4 instead of multiplying by 25. This was seldom possible however. Occasionally she noted that the answer seemed to come automatically, in one process without consciously thinking through the steps. "It opened out before me." C. was specializing in the mathematics department, so was probably better prepared with devices and automatic calculations than any of the others.

In scoring this test, errors were penalized by adding on .2 of the time taken for 1 error, .3 for 2 or 3 errors, .4 for 4 or 5 errors, and .5 for 6 errors in the final answer. As it happens, subjects who are usually accurate seem doubly penalized by this, since with them the consciousness or suspicion of error lengthens their time in any case, whereas with the habitually inaccurate an error more or less made no appreciable difference in the time taken.

Records for each of the 60 examples were kept to see if any particular one was much more difficult or easy than the rest; but both good and bad scores were made with almost every example, and none could be singled out as specially difficult or easy.

In the table that follows the daily average score for each subject is given, that is, the average score on three examples for 20 days. The curves as plotted from them are in Fig. 5.

As each point on the curve represents an average rather than a single trial, the curves may be considered partly smoothed already. The two most regular and most alike are those of C. and Sa.; the most irregular is that of Sch.; the most unlike any other is that of H., though after the sixth day when her scores are within the range of those of the other subjects, her curve is more regular, and not unlike E.'s or Jb.'s.

The curves representing separately the factors of speed and accuracy are shown by a continuous and a dashed line respectively in Fig. 6.

From this it will be seen that there is very little if any improvement in accuracy, but a good deal in speed. Also, of the most accurate subjects, H. is the slowest, Nb., C., and Sa. are the quickest. Also that there is more individual difference revealed in speed than in accuracy, judging by the amount and regularity of improvement in each.

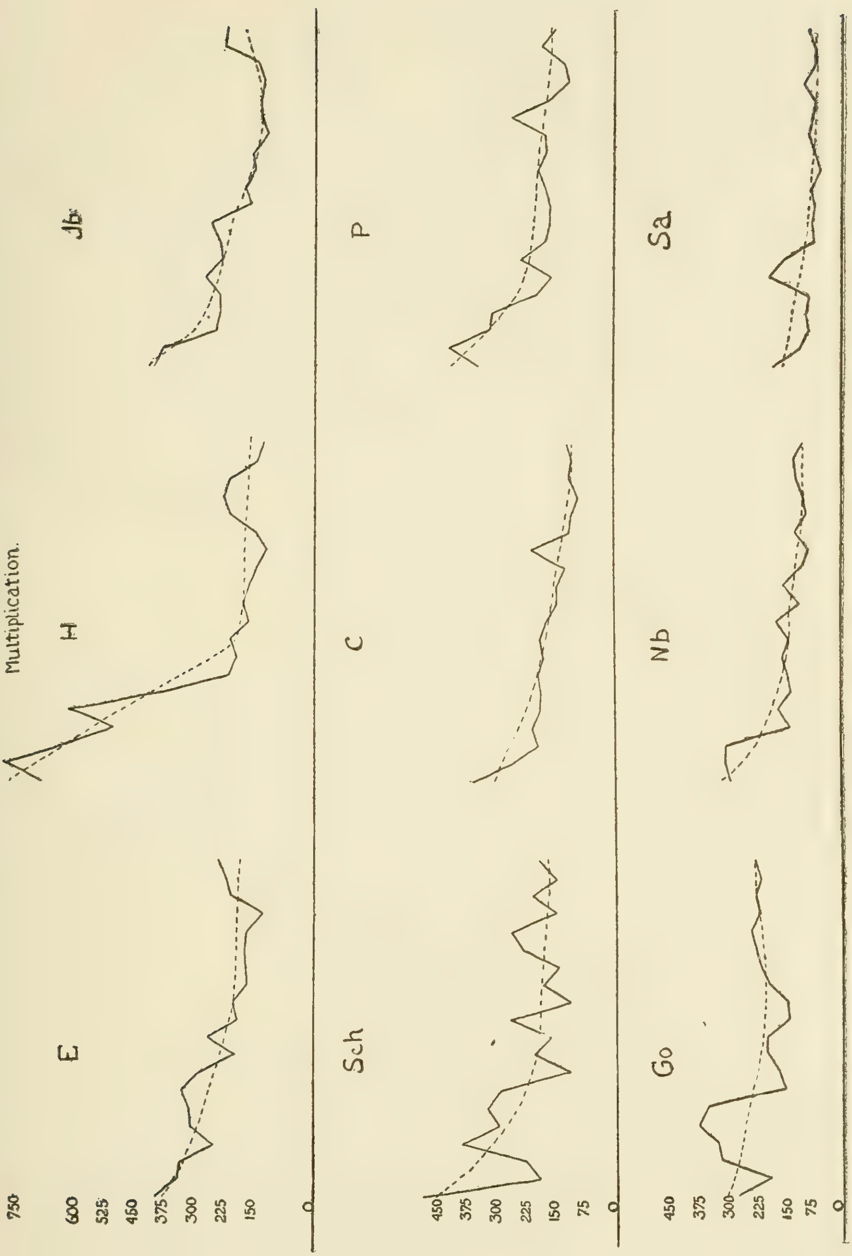


Fig.5.
Multiplication.
E
H
Jb
Sch
C
P
Go
Nb
Sa
750
600
525
450
375
300
225
150
0
450
375
300
225
150
75
0
450
375
300
225
150
75
0

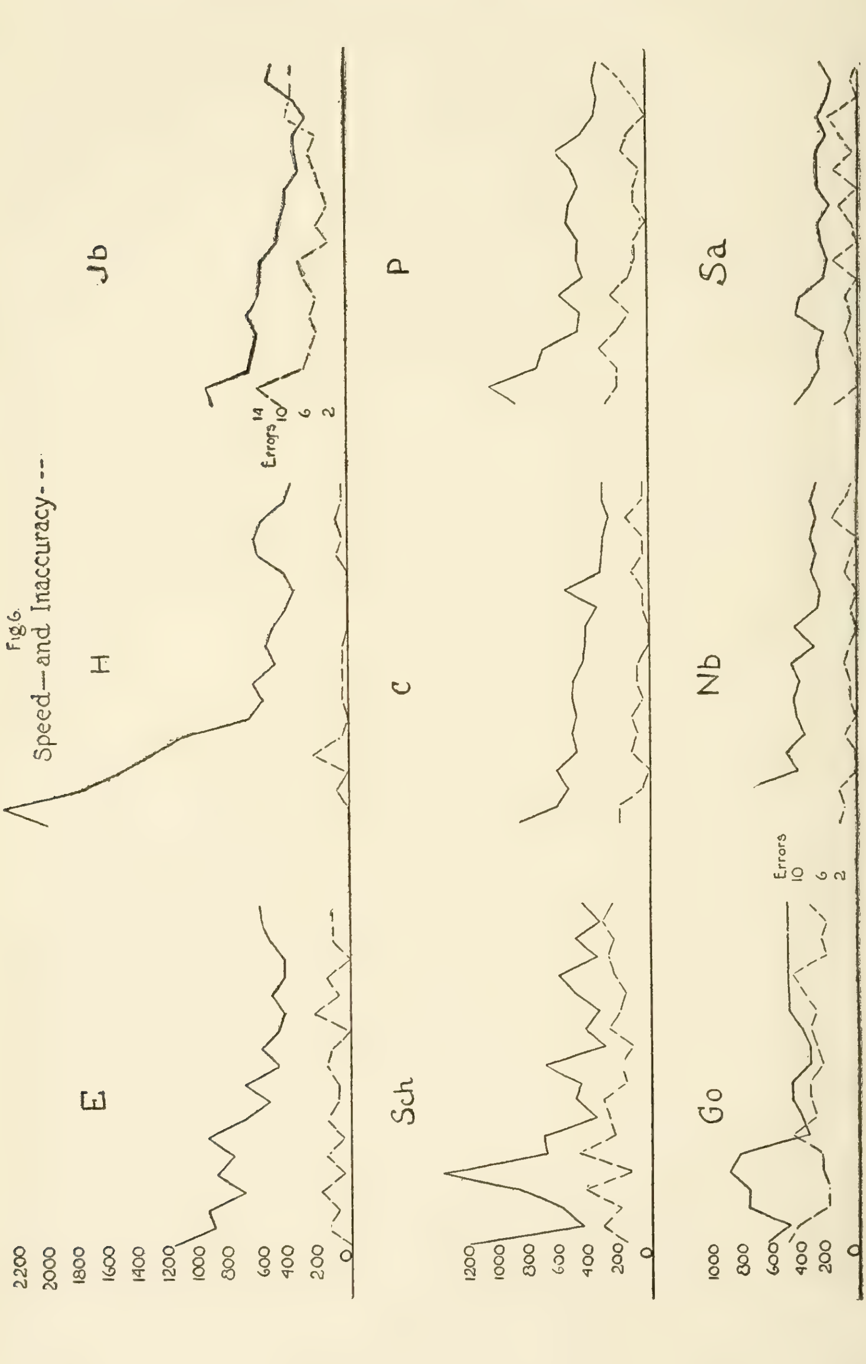


Fig. 6.
Speed—and Inaccuracy- - -
E
H
Jb
Sch
C
P
Go
Nb
Sa
Errors 14 10 6 2
Errors 10 6 2
2200 2000 1800 1600 1400 1200 1000 800 600 400 200 0
1200 1000 800 600 400 200 0
1000 800 600 400 200 0

TABLE LII

SCORES IN MULTIPLICATION TEST

Day	E.	H.	Jb.	Sch.	C.	P.	Go.	Nb.	Sa.	Average
1	400	685	408	492	360	344	284	303	189	389
2	348	790	382	196	262	419	218	310	119	338
3	344	642	251	234	198	315	332	308	97	302
4	255	510	242	395	207	307	338	145	108	282
5	316	628	240	303	189	193	380	175	99	280
6	320	389	278	328	187	160	356	142	203	262
7	340	220	235	289	191	240	164	150	165	221
8	294	209	240	117	180	174	182	166	85	187
9	204	227	260	214	184	165	214	146	88	189
10	276	176	168	166	168	160	210	182	81	176
11	196	184	180	275	145	174	156	113	93	169
12	212	170	154	116	145	192	156	164	69	152
13	175	145	155	193	121	172	208	110	84	151
14	176	120	121	147	216	176	234	95	98	153
15	175	145	135	239	109	261	240	130	94	170
16	166	219	141	278	96	172	249	111	83	168
17	140	229	116	153	76	113	228	110	108	142
18	216	211	147	217	96	126	234	127	82	161
19	225	145	233	148	94	188	228	132	81	164
20	242	131	225	195	96	133	237	110	97	163
Average ..	251	307	216	235	166	212	242	163	106	

Below are the rankings of the nine given as for the other three tests considered so far. Jb. and E. are perhaps penalized here as their last few records were worse than say the fourteenth and fifteenth. Otherwise the correlations would all be closer. It must be remembered too that the steps in the *speed* ranking are much more unequal than in some of the other tests.

TABLE LIII

	Position at Start (1 = Least Time)	At Finish (1 = Least Time)	Av. Position (1 = Least Time)	Speed (1 = Least Time)	Accuracy (1 = Fewest Errors)	Gross Gain (1=Most)	Per cent. Gain (1 = Most)
E.	6	7.5	8	8	5	7	7
H.	9	4	9	9	1	1	1
Jb.	8	9	5	4	8	6	6
Sch.	5	6	6	7	7	5	5
C.	4	2	3	2	4	3	2
P.	7	5	4	5	6	2	4
Go.	2	7.5	7	6	9	8	8
Nb.	3	3	2	3	2	4	3
Sa.	1	1	1	1	3	9	9

The correlations are:

Position at start and at finish .... $R = .44$
Position at start and average position................ .58
Position at start and gross gain..................... —.63
Position at start and per cent. gain.................. —.52
Speed and accuracy .... .10

The same general conclusions would be drawn as for the other tests, except that there is a slight positive relationship between speed and accuracy. Possibly the quasi-automatism in the familiar arithmetic processes noticed by C. may account for this.

In the *maze test* the scoring was done—as with other subjects—by adding .1 to the time taken for 1 or 2 touches, .2 for 3 or 4 touches, .3 for 5 or 6 touches and so on. The daily scores resulting are given below and the curves plotted from them in Fig. 7.

TABLE LIV

E.	H.	Jb.	Sch.	C.	P.	Go.	Nb.	Sa.	Average
216	180	174	118	165	170	288	165	172	194
204	195	264	121	198	154	240	174	333	209
180	180	143	117	187	165	90	180	280	169
216	180	159	150	198	130	306	180	195	190
192	165	121	135	181	148	96	180	273	166
168	165	154	153	160	143	192	132	290	173
216	135	176	89	181	165	228	148	259	177
216	150	224	117	190	135	228	161	281	189
195	150	120	132	160	182	264	144	215	173
168	150	221	117	209	152	108	168	244	171
132	181	108	84	176	140	144	158	316	159
168	198	187	154	140	128	108	156	247	165
144	181	120	100	160	130	132	132	203	145
144	180	100	55	190	132	108	135	210	139
126	180	142	156	130	139	114	110	212	145
168	165	88	121	140	182	108	88	210	141
132	198	120	89	120	115	108	108	190	131
144	150	168	96	149	135	102	117	231	143
144	148	99	96	143	165	120	135	231	142
144	120	90	144	165	144	102	120	82	123
Av. 171	162	148	117	162	147	159	144	238	

It must be remembered that these are only single trials; also, from experience with other subjects, notably the long-term group and R. and Wy., that a conscious attention to speed is accompanied by decreased accuracy. No track was kept by these nine subjects as to whether they attended more to speed or to accuracy. The oral directions emphasized the latter, but the general conditions of the test—timing themselves and having to enter the time—would probably emphasize the former. From these facts then very irregular curves would be expected, which is exactly what is shown.

Go.'s apparent regularity in the second half is due partly to her careless entries of whole minutes, partly to her consistently high number of touches. H.'s comparative smoothness is due to her almost perfect record for accuracy. When these curves are smoothed out C. and P. are most alike, Sch. and Sa. most unlike.

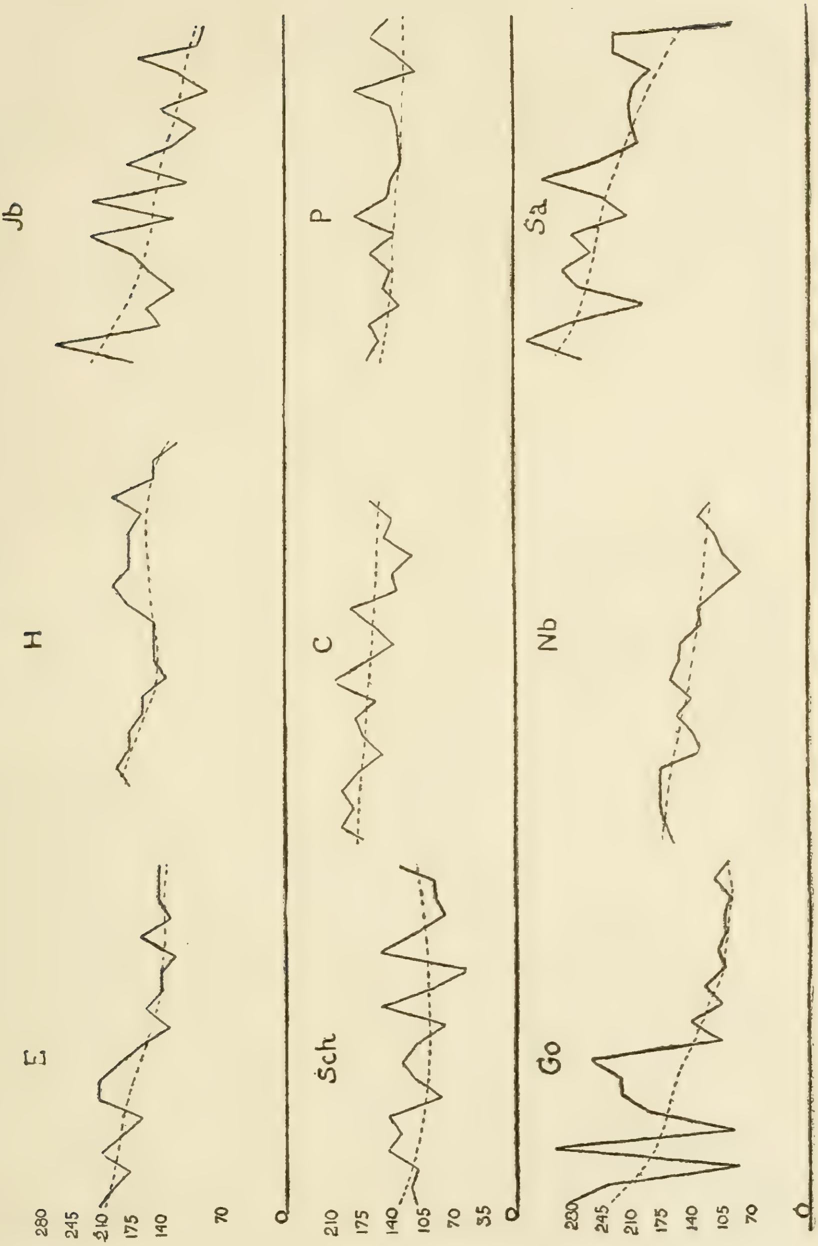

E
H
Jb
Sch
C
P
Go
Nb
Sa
280
245
210
175
140
70
0
210
175
140
105
70
35
0
280
245
210
175
140
105
70
0

Fig. 7
Maze

The rankings are given below as for the other tests, and also the correlations worked out from them.

TABLE LV

	At Start (1=Least Time)	At Finish (1=Least Time)	Average Position (1=Least Time)	Speed (1=Least Time)	Accuracy (1=Fewest Touches)	Gross Gain (1=Most)	Per cent. Gain (1=Most)
E.	5	6	8	4	8	5	5
H.	4	5	6.5	8	1	6	6
Jb.	6	1	4	5	5	3	2
Sch.	7	3	1	2	7	4	4
C.	3	7.5	6.5	7	3	8	8
P.	1	7.5	3	6	4	9	9
Go.	8	2	5	1	9	1	1
Nb.	2	4	2	3	6	7	7
Sa.	9	9	9	9	2	2	3

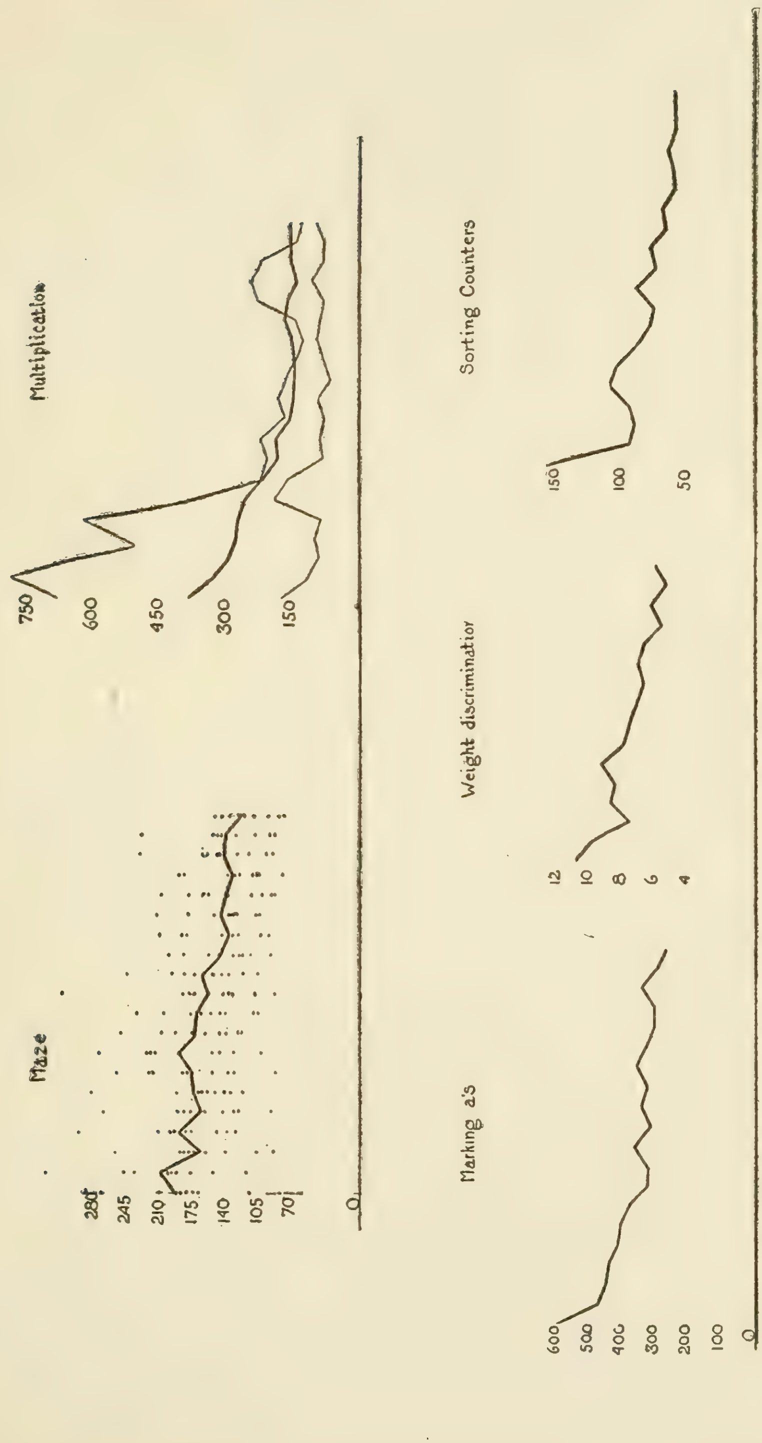
The correlations are:

Position at start and at finish	$R = -.21$
Position at start and average position..............	.33
Position at start and gross gain....................	—.95
Position at start and per cent. gain................	—.90
Speed and accuracy	—.93

In this test the subjects do not keep their relative positions through the series; and, as might be expected, speed and accuracy are almost completely inversely correlated.

Now to examine the data for answers to the questions raised: first, is a mean curve for a test representative of the test or do individual curves differ too much from it and each other to make it reliable? After all, since any average tells little unless accompanied by a statement of the variability, and since a curve of practise is nothing but a series of such non-significant averages, one would not expect a mean curve to be representative of anything beyond the fact of change. Still, the changes in rate of improvement as shown by the mean curve may be different with different functions, or there may be one typical curve of practise to which all functions approximate. In Fig. 8 are shown five mean curves, one for each test. That for the maze is accompanied by a scattering of dots to show the distribution of the nine around each average point; that for mental multiplication is accompanied by the two most distinctly different curves, those of H. and Sa. to show the range. Without these representations of variability there is nothing to distinguish one curve from the others. All alike show greater improvement near the beginning and only slight irregularity after about the seventh day.

Fig. 8.
Mean Curve. For Each Test.



All the functions do seem to approximate one typical law for changes in the rate of improvement.

The second question, are the changes in the rate of improvement different with different individuals or is there one typical curve of practise to which all individuals approximate is answered so far as these data go by Fig. 9. In this are shown the nine sets of smoothed curves, one for each individual. Those for C. and P. are different from those of H. and Go., the former being level and smooth, the latter with a sharp slant near the beginning. Sa. also belongs to the former group, only her relative position in the various tests is very different. Jb. and Nb. show a moderate slant in practically all; E. and Sch. have a mixture of types. This may mean that practise does disclose easily recognizable individual differences, that some people improve rapidly at first, others at about the same rate all the time. Or it may mean only that giving a few trials shows at the beginning a great range of abilities and that the range is lessened with practise. Those who are poor in ability have the greatest leeway to make up and so improve rapidly, while every one improves rather slowly once a certain degree of ability is reached. Thus if comparison is made after the sharp initial slant is over, individual curves will resemble each other in form very closely. In general it seems most probable that if all individuals could start with absolutely zero practise and their changes in rate of improvement up to the limit of improvement be measured, that their curves would resemble each other very closely. The apparent differences as found are so largely caused by the very different levels at which they start, as well as to chance variations in their daily performances.

Individual differences do however occur in the consistency of performance shown by the relative freedom from irregularity in the slope of the curve. If the irregularities of C. and Sa. on the one hand and of E., Go., and Sch., on the other, were computed, the general tendency of the three last to more irregular progress than that shown by the former two would be found much greater than would be expected by chance. This difference is, however, simply one form of the general differences in variability of performance, not anything peculiar to the learning process by itself.

However, since all C.'s curves are not alike, nor all Go.'s, it may be that there is some truth in the third condition suggested, namely, that a curve reveals special not general ability in an individual. That is, that in some kinds of work an individual who is good in any case when compared with others will make steady though slight improvement, while one who is relatively poor will either improve rapidly at first and irregularly for a considerable period, as Sch. in

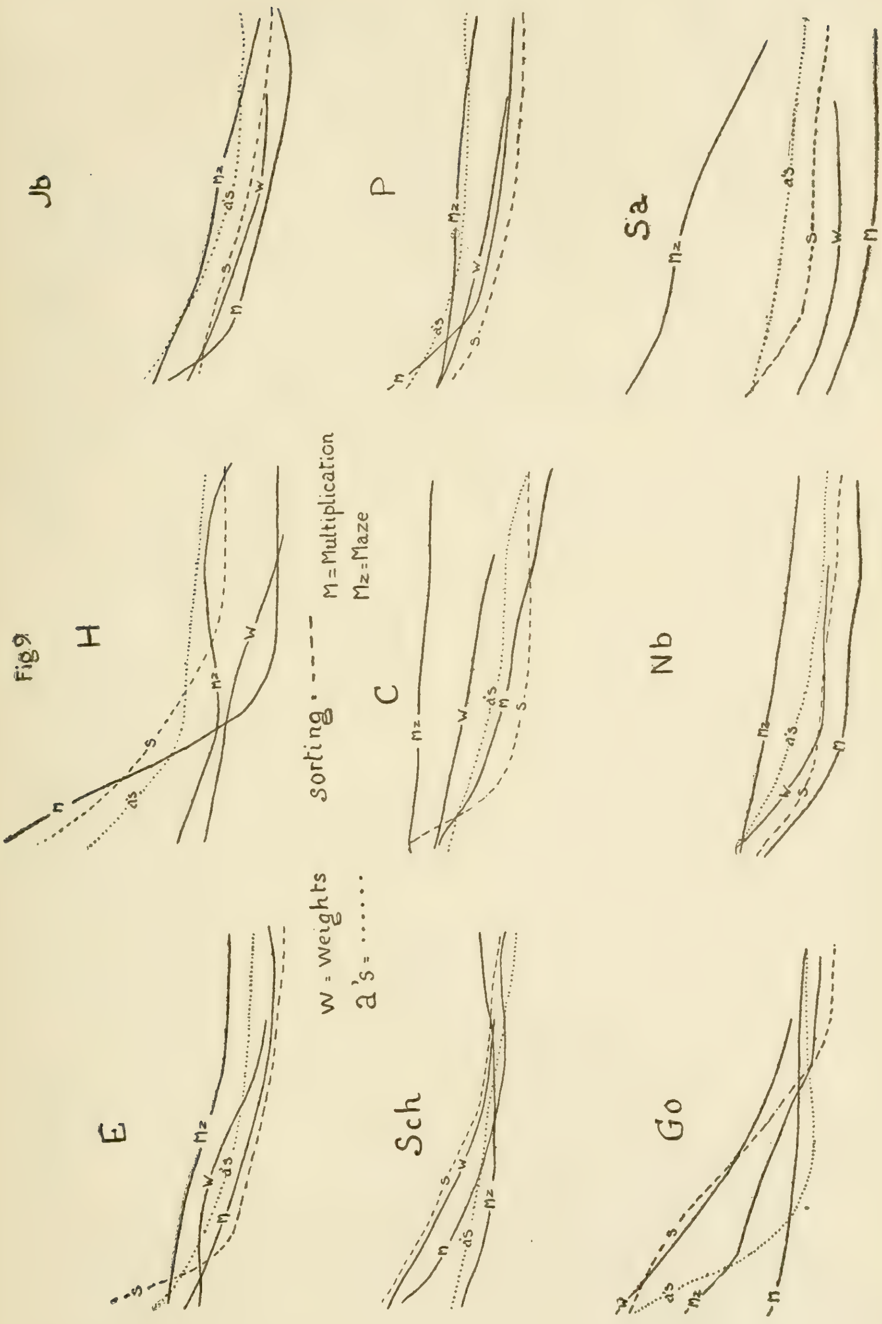

Jb
H
E
P
C
Sch
Sa
Nb
Go
W = Weights
a's = ...
sorting - - - -
M = Multiplication
Mz = Maze

Fig 9

mental multiplication, or he will improve very little if at all, perhaps regularly as E. in the maze but more likely irregularly, as E. in weights and sorting, and Sa. in the maze.

In other kinds of work the individual's initial ability may be relatively very different but his tendency toward great irregularity in practise or the reverse may persist. Even in a test such as judgments of lifted weights where all nine curves are more or less irregular, those from C. and Sa. and perhaps Nb. who were notably regular in the other tests are less irregular than those from E., Go., and Jb., who were irregular in other tests as well.

Finally, if irregularity is disregarded and all curves smoothed out, only those facts conforming to the "law of the practise curve" are represented, namely, that a person improves in any work most rapidly at first and makes little and slow improvement after reaching a certain degree of ability. From this point of view, since smoothed mean curves resemble each other no matter whence their derivation, practise must tend to make people more alike.

IV

CONCLUSIONS

Reviewing this experimental study as a whole, it may be said to offer evidence in reply to certain criticisms of the method of mental tests.

1. In the first place the kind of tests given are said to be of little significance, that knowing how many A's an individual can cancel in a given time, or how many objects he can sort or how many opposites he can name tells us very little about him. This is probably true to a certain extent, since the simpler the performance the more alike individuals will probably be. Complex processes from real life may often be more significant but are necessarily less precise, less convenient, less well recorded and scored, and may therefore be limited to the descriptive stage of investigation. Making more precise measurements need not exclude descriptive work, however, for, in individual tests at least, details of temperament, speed in responding, intelligence in understanding and following directions can be noted, while in addition there will be the objective record to serve as basis of comparison. Then too, with careful experimentation, the tests proven most typical or significant can be selected and administered in the best way. For instance, the easy opposite test given by the time-limit method seems to be a truer measure of the speed of association than the first-idea test by the amount-limit method. The straight maze if improved with respect to length and continuity of movement would probably be more significant and precise as a measure of speed and accuracy of movement than is the hitting 100 dots.

2. In the second place, the criticism that a single trial is unreliable is true but need not be exaggerated since other facts such as state of fatigue, time of day, temporary embarrassment, inclination for work and familiarity with the environment and the kind of material used also enter in to make trials unreliable. To overcome this in part, at least two trials should be made of any test, preferably in addition to a few minutes fore-exercise in similar work. Fewer tests each administered oftener would give a truer estimate of an individual and a better basis for comparison and correlations. It might be advisable to allow sufficient time for each test to get the

average divergence of the obtained result for an individual from the true result down to some standard of reliability agreed upon by various investigators.

3. In the third place, the criticism that giving only a few trials measures not the mental process supposedly tested but merely adaptability to strange conditions such as apparatus, instructions, working for speed, and the particular requirements of the test is seldom of weight. Early improvement due to this alone is rare, and even so could be checked by proportionate fore-exercise and the choice of a proper test.

4. In the fourth place, the criticism that tests measure the degree or amount of previous similar experience rather than actual capacity is true not only of such tests but of any form of mental measurement. It should operate only against expecting too much from the tests, not against their use, but rather, in fact, in favor of repeating them at stated intervals. The only alternative—testing subjects with no similar previous experience or else those whose training had brought them to the physiological limit—would be impracticable, and out of the question. In general, tests of a novel, little-trained function such as *grouped objects* or the *a — t* test show greater susceptibility to practise than those of a frequently used, much trained function such as addition.

5. In the fifth place, in estimating the nature and degree of improvement in a function with repeated trials the nature of the units used to express such improvement must be taken into considération, and misleading statements based upon one form of measurement only must be guarded against. Moreover, when comparisons of changes are to be made, whether between different processes in an individual or a group, or between different individuals in one process, it becomes still more important to use more than one way of treating measurements.

6. In the sixth place, the criticism that practise may influence individuals each by a law of his own and processes each by a law of its own does not seem to hold so far as the general law of improvement goes. On the whole, higher mental functions are sooner susceptible to practise than are sensory functions, the more so again if they are novel. Individuals with low standing can and do improve the most, judging objectively, though even so they may not, in conveniently measurable periods of time, overtake those whose standing was high at the beginning. Characteristic variability or consistency of performance may be disclosed whatever the process and whatever the change in improvement.

APPENDIX

Key for Correction of Opposites

Right, scored 2. (Second choice, scored 1.) *Wrong scored 0*

Above	Below, beneath, *under, down*
Absent	Present (here)
Adroit	Awkward, clumsy (unskilful, unskilled)
After	Before (ahead)
Apart	Together (with, near)
Asleep	Awake
Backwards	Forwards (frontwards)
Barbarous	Civilized (humane), *tame, cultivated*
Best	Worst
Big	Little (small)
Bless	Curse
Broad	Narrow (thin)
Broken	Whole (mended, unbroken, intact)
Brother	Sister
Buy	Sell
Cheap	Dear, expensive
Clumsy	Adroit, deft, skilful, neat (adept, agile, graceful), *clever*
Come	Go
Country	City, town
Create	Destroy, annihilate, tear down (abolish, spoil)
Day	Night
Dead	Alive, living
Deceitful	Sincere, straightforward (truthful, honest, frank, candid, honorable), *open, true, ingenious, upright*
Degrade	Elevate (exalt, uplift, raise, ennoble, promote, advance, restore, honor)
Diligent	Lazy, indolent
Elation	Depression, dejection (despondency, low-spiritedness)
Enrage	Pacify (subdue, appease, calm), *quiet*
Exciting	Depressing, quieting, soothing (calm, restful)
Expand	Contract, condense (decrease, narrow), *enclose*
to Float	Sink (anchor)
Forcible	Weak (gentle), *gently*
Frequently	Seldom, rarely (not often, occasionally)
Generous	Stingy, parsimonious (miserly, greedy, mean), *avaricious*
Gentle	Rough (rude, harsh)
Genuine	False, spurious (counterfeit, sham, insincere, artificial, unreal, imitation, fictitious), *fake, bogus, adulterated, spurious*
Grand	Simple, trivial (poor, petty, modest, ordinary, humble, mean, ignoble, plain, commonplace, insignificant), *tawdry, mediocre, lowly*
Here	There
Hinder	Help, aid, further (promote, advance, assist, hasten, quicken)

Hold	Let go, release, drop (lose, give up, loosen), *give, loose*
If	Unless (although, certainly)
Ignorant	Wise (informed, learned, knowing, educated, intelligent)
to Lack	Have, possess, abound (have in abundance, gain), *attain*
Land	Water (sea)
Less	More
Level	Uneven, slanting, sloping, inclined (rugged, hilly, mountainous, irregular, undulating), *jagged, rough, bumpy, broken*
Loquacious	Taciturn, silent (quiet, reticent, reserved)
Mine	Yours (his, theirs), *your*
Motion	Rest (still, standstill, stillness, quiet)
Obscure	Clear, lucid (plain, evident, light, bright), *open, significant*
Over	*Under* (below, beneath)
Part	Whole, meet (totality, entirely)
Past	Future (present)
Permanent	Temporary (transitory, transient, fleeting), *ephemeral, evanescent, unstable, changing*
Permit	Forbid, deny, prohibit (prevent, refuse), *hinder*
Precise	Inexact (careless, slovenly, disorderly, lax, indefinite, vague, inaccurate), *irregular, loose*
Proud	Humble, *cosmopolitan, democratic*
Repulsion	Attraction, *liking, encouragement, acceptance*
to Respect	Despise (look down on, disregard, insult), *abhor, scorn, loathe, dislike*
to Reveal	Conceal, keep secret (hide, obscure, cover up, keep back)
Rough	Smooth, gentle (calm, tender), *easy*
Rude	Polite, civil, courteous (cultured, sophisticated, obliging, gentle), *refined, fine, polished*
Separate	Together, combined, meet, join, connect (collective, united, continuous)
Serious	Frivolous, gay (merry, laughing, joking, jocular, mirthful, lively), *jocose, funny, silly, cheerful*
Simple	Complex (hard, wise, clever, complicated, difficult, intricate, profound, elaborate)
Son	Daughter (father)
Spend	Save (keep, hoard), *hold*
Stormy	Calm (clear, quiet, fine, peaceful, smooth, tranquil), *fair, mild*
Straight	Crooked (curved)
Stupid	Sensible, bright, clever (smart), *wise, alert*
Take	Give (leave, let alone)
Tall	Short
Unless	If (in spite of, though), *because*
Vertical	Horizontal (slanting), *crooked, perpendicular*
Weary	Fresh (refreshed, rested, brisk, lively), *energetic*
Wicked	Righteous, good (holy)
Wild	Tame, cultivated (civilized)
Win	Lose

PARAGRAPHS USED IN THE EBBINGHAUS COMBINATION TEST

I–XX were specially prepared for the long-term group. The remaining paragraphs, prepared by other investigators, were used with the short-term group.

I.

The argument amounts .. this, that like consequents must .... like antecedents. But it is impossible for the antecedents to be .... alike, in that the thoughts and feelings ..... give rise to my movements are immediately given, while ..... which give rise to ..... people's movements are ... given. The question presents ......, whether this essential .... in the mode of existence .. the antecedents does not wreck the analogy.

II.

From the facts thus ... presented, it would be natural to infer .... mind and body are, in respect of action, on a footing .. equality. The interactionist, at this point, might be tempted to set up the ..... that every fact showing the influence of .... upon mind can be matched with a .... showing ... . of .... upon ...., and that by as much as the former demonstrates the mind's dependence, the ...... demonstrates its power.

III.

In every actual case of perception, the entire fact is not ...... the presence of a physical ...... to consciousness, but at the same ...., and as a condition of that presence, the existence of a train of .... and effects connecting the object .... the percipient's ...... If I .... a table, this involves the presence in the .... world, along with the table, of light-rays passing from the ..... to the eye, and .... passing from the eye to the brain.

IV.

Parliament had hitherto .... very little attention on our Eastern possessions. Since the death of George II., a rapid .... of weak administrations each of ..... was in turn flattered and betrayed by the Court, had held the .... of power. Intrigues in the palace, riots in the capital, and insurrectionary .... in the American colonies had left the advisers of the Crown little time to study Indian politics. When they did interfere their interference was ...... and irresolute. Lord Chatham had .... a bold attack on the Company, but his plans were rendered .... by the strange malady which about that ..... began to overcloud his splendid genius. At length it was generally felt that Parliament could no longer .... the affairs of India.

V.

Very similar to this was the state of India sixty years .... Of the existing governments not a single one could lay ..... to legitimacy. There was scarcely a province in which the real sovereignty and the .... sovereignty were not disjoined. Titles and forms were still .... which implied that the heir of Jamerlane was absolute ..... when in reality he was a captive. The Nabobs were, in some ...... independent princes; in others, they had, ... their master, become ..... phantoms and the Company was supreme. Among the Mahrattas the heir still .... the title of Rajah; but he was a prisoner, and his prime minister had ...... the chief of the state.

VI.

In a rude state of society men are children with a greater variety of ideas. It is in such a state of society that we may ..... to find the poetical temperament in its .... perfection. In an enlightened ... there will be much

intelligence, much ...., much philosophy, abundance of just classification and subtle ...., abundance of wit and eloquence, abundance of verses and even of .... ones; but little .... Men will talk about the old poets and comment on them, and to a certain extent ..... them, but they will scarcely be able to .... the effect which poetry produced upon their ruder ...., the ecstasy, the plenitude of belief.

VII.

One of his gifts was a voice habitually deep and sonorous yet capable of .... very low and gentle at the ...... moment. About his ordinary bearing ..... was a certain fling, a fearless expectation of success, a confidence in his own ...... and integrity much fortified by contempt for .... obstacles or seductions of ...... he had had .. experience. Mr. B. perhaps liked him the ...... for the difference between ...., and certainly for being a stranger. One can begin so many things with a ... person!

VIII.

He had never put any question concerning the nature of his illness, nor had he betrayed any .... as to how far it might be likely to cut ..... his labors or his life. On this point, as on all others he ...... from pity; and if the suspicion of being pitied for anything surmised or known in ..... of himself was embittering, the idea of calling ..... a show of compassion by frankly .... an alarm was intolerable. Every proud mind knows something of this .... and perhaps it is only to be .... by a sense of fellowship deep ...... to make all efforts at isolation seem mean and petty .... of exalting.

IX.

Her belief that Rosamond could manage her papa was well founded. Mr. Vincy had as ..... of his own way as if he had been a prime minister: the force of .... was easily too .... for him as it is for most pleasure-loving, florid ...; and Rosamond was .... forcible by means of that mild persistence which enables a soft living substance to make its ... in spite of opposing rock. Papa was no rock. He had no fixity but that of alternating impulses sometimes ..... habit, and .... was altogether unfavorable to his taking a decisive line of .... in relation to his .... engagement.

X.

Soldier wake, the ... is peeping
Honor ne'er was ... in sleeping,
Never .... the sunbeams still
Lay unreflected on the ....:
'Tis when they are glinted ....
From axe and armor, spear and jack,
That they promise ...... story
Many a page of deathless .....
Shields that are the foeman's terror
Ever ... the morning's mirror.

. .

Soldier, ..., thy harvest, fame;
Thy study, conquest; war, thy ....

XI.

And is she happy? Does she see unmoved
The .... in which she ..... have lived and loved
Slip without .... bliss slowly away,
One after one, ... like to-day?
Joy has ... found her yet, nor ever will,
Is it this .... which makes her mien so still
Her features .. fatigued, her eyes, tho' sweet,
So sunk, so rarely ...... save to meet
Her children's? She moves slow; her voice alone
Hath yet an infantine and silver tone,
But .... that comes languidly: in truth
She ..... one dying in a mask of youth.

XII.

Move eastward, happy earth, and leave
Yon orange ...... waning slow;
From fringes of the ..... eve
O, happy planet, ... go;
Till over thy dark shoulder glow
... silver sister ....., and rise
To glass herself in dewy eyes
That ..... me from the glen below.

Ah, bear me with ...., lightly borne,
Dip forward under ...... light
And move me to my marriage ....
And round ..... to happy night.

XIII.

Professor Crocker presented his trained animals yesterday afternoon and .... and was greeted .. large houses on both .... The production is unique and ...... an interesting lesson in .... education, some .. the tricks .... by the four-footed actors being really .... His troup consists of 25 animals, and .... has a role to .....

XIV.

Weather that was pleasant only at times, and at ..... times threatening or rainy made ...... unpleasant conditions ... yesterday's observance of Dominion day, and .... a damper on many festivities. The morning dawned bright and ..... and scores of ...... parties left the city on excursions. Towards noon it became cloudy and there were some .... Again it cleared up, only to be .... later by heavy thunder, lightning and rain, though the .... in the city was light .... to what it was in the surburbs.

XV.

The longshoremen of the Cunard ..... pier who struck yesterday ..... the steamship Umbria arrived to ...... the company to pay them sixty ..... instead of fifty-five cents an hour for Sunday ....., returned to work to-day. Their demand was not .... The chairman of the ..... said to-day that he was at a loss to .... the reason for the action of the .... men. He said the union did not .... the strike.

XVI.

The magnetic dip needle is made in the form of a lozenge, ... to the horizontal needle, but it is poised or .... by ...... of a shaft running through the center of the lozenge at right ...... to it, and is held in .... by agate bearings as ...... in figure 20. In some types the cradle .... the horizontal shaft is poised on a steel needle. The needle is thus ..... to take up a position ...... and south and to incline on its ......

XVII.

It is natural to believe in great men. Nature seems to ..... for the excellent. The world is upheld by the veracity of .... men; they make the earth wholesome. They who lived .... them found life glad and nutritious. Life is sweet and tolerable only in our belief in .... society; and actually, or ideally, we manage to .... with our superiors. We call our children and our lands by their ...... Their names are .... into the verbs of language, their works and effigies are in our ......, and every circumstance of the ... recalls an anecdote of them.

XVIII.

If he had been an English nobleman on a pleasure tour, or a newspaper courier, he could not have .... more quickly. The post boys wondered at the fees he ...... amongst them. How happy and green the country .... as the chaise whirled .... from milestone to milestone, through neat country towns where landlords ..... out to welcome him with ...... and bows; by pretty roadside inns where the signs .... on the elms, and horses and men were drinking under the checkered ...... of the trees; rustic hamlets .... round ancient grey churches, and through the friendly English landscape. To a traveller returning ..... it looks so kind.

XIX.

Nay, ye should not weep, my children!
Leave it to the faint and weak;
Sobs are ... a woman's weapon
Tears befit a maiden's .....
Weep not, ... of MacDonald!
..... not thou, his orphan heir.
Not in shame, but .... honor
Lies thy slaughtered ...... there.
Weep not, but when years are over
And thine arm is ...... and sure,

. . . . .

Let thy heart be ..... as iron
And thy wrath as fierce .. fire,
Till the hour when .... cometh
For the race that slew thy sire!

XX.

An electrical storm of ..... severity passed over this district last night, which burned barns, killed cows in the field, put telephones and .... lines out of commission, knocked ..... trees, and did a great deal of .... generally. The flag staff was struck and splintered and the slates were .... off the

roof. A barn was burned with a large .... of hay, and a driving shed was .... destroyed. Crops in all .... were almost pounded into the ....

XXI.

We confess to something of sympathy ... the correspondent ... hinted yesterday that ... children are ... over and killed by automobiles, the .... is not always that .. the automobilist, ... sometimes rests in some measure on those who do not ..... their children to avoid unnecessary ...... It is a plain ...., of course, that public highways are ... the use of the whole population, ... that the automobilist is ..... every obligation .. keep the limitations of his rights and privileges .. mind as he goes along, but the road is his .. well as other peoples.

XXII.

If we are .... well, thoroughly sound, we .... not be depressed. The perfectly healthy animal ... no worries. The remedy has already .... indicated. Regretfully it is .. simple .... very few people take the trouble to ..... it. .... it is clearly and widely recognized that ..... is stupid, that its .... is simple where ..... is no organic trouble, worry will ...... Worry is simply a .... of what, ... the sake of a nice large word, is called ''neurasthenia,'' nerve-depletion. ..... plenty of recreation, plenty of fresh air, and the ...... man will not worry.

XXIII.

Park Hill on the Hudson offers you a solution .. the home problem to-day. No home seeker .. investor ... afford to ignore its claims. Escape the wear and tear .. the city's noise ... rush .. this open air paradise, just .. the city's edge, .. all respects an ideal home location ... yourself and family. ..... are cottages containing every improvement waiting ... you to step .. and make yourself comfortable. It not .... commands the most beautiful view around New York ... is protected for all time .... intrusion. Choice lots now .... on very easy terms.

XXIV.

A law .. defence of property rights in the broadest sense .. observed ..... almost abolish international conflicts. Gentlemen .. not fight with fists .. money differences ... do they refer them .. courts of honor. Civil courts are for that .... and are as useful for nations as for men. The sanction of international law must .. merely moral, for a long time .. least. But in ...... that there should be ... moral sanction there must .. a moral code. The principles of .... a code are deducible .... treaties to which nations have set their hands ... seals.

XXV.

I asked the slovenly, ... cheerful female ... answered the bell ... the landlady, wondering the while .... I should say when I was asked ... references. The merriment had not been called forth .. anything amusing .. my appearance, .. my vanity had feared, ... by a story which a man sitting .. ... head of the table was just finishing. The only vacant chair .. the room was beside him, and, rather awkwardly, ... I felt that they were ...... my measure, I made my ... toward it. As I ... down he greeted .. with a polite bow.

XXVI.

The occult in everyday affairs is the ..... of this new book .. Robert Chalmers. .... one of the thrilling stories of ..... the volume is composed .. the tale of some awful mysterious happening, some supernatural ..... beyond the ..... of material reasoning of mortal man .. explain, which comes .... the life of some ordinary, everyday man. The opening .... tells of a dinner ..... to a man deeply versed in occultism .. his American friends. To these he gives many hints ... suggestions of momentous things which he ... plainly see .... for them .. the future.

XXVII.

We believe we can prove .. you that this investment is .. secure ... the dividends so sure, that it justifies you .. withdrawing money .... the Savings Banks, ... it is earning $3\frac{1}{2}\%$ and putting it .. our business where it will earn 7%. We are a New England enterprise, managed .. New England men, and we have behind .. a record .. fourteen years of unbroken success. .... you have much or little you can not ...... to let slip this opportunity of doubling the ...... from your savings. Prompt action in this matter will ..... you well.

XXVIII.

On the ...., it didn't cost me a dollar. In fact, though at ..... I have found myself .... of considerable sums of ready money, I have never .... a man of property .. the strict sense of the word. I abandoned my ...., the law, .. I did not .... its practice so lucrative .. I had hoped. For some years thereafter I traveled largely .. the Mississippi River. It ... the decline in steamboating ... the adoption .. less leisurely methods of travel .... cut into my income and forced .. to come North and ...... in trade.

VISUAL ACUITY WITH LIGHTS OF DIFFERENT COLORS AND INTENSITIES

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VISUAL ACUITY WITH LIGHTS OF DIFFERENT COLORS AND INTENSITIES

I

INTRODUCTION

Although the problems of vision in general, and in particular those of the comparative luminous and physiological effects of the different colors of the visible spectrum, have for many years claimed a large share of the time and thought of investigators in the fields of physics, physiology and psychology, attention has been centered largely upon theoretical considerations. Little effort has been made to give practical application to the principles of color vision which have been established as a result of these investigations, except in so far as they have had reference to esthetics and pictorial art. The artist knows very definitely what combinations of colors should be made in order to produce the effects he desires, but on the other hand the average person of intelligence knows only in the most general way, if at all, what intensities of illumination or what colors of light are best adapted to the purposes of ordinary vision.

This general indifference to the subject of the luminous efficiency of different sources of light has been due largely to the fact that until comparatively recent years the range of intensities and colors available for purposes of artificial illumination has been very much restricted. With the exception of the oxy-hydrogen flame, which is too inconvenient and cumbersome for use outside of the laboratory, the only artificial sources of light available were the incandescent vapors of oils and the ordinary illuminating gas. As both these lights are of approximately similar hue, the question of color differences was never raised, and inasmuch as the intrinsic brightness of each is very low, the only problem involved in their use was that of getting sufficient light from them. Excessive cost and physical limitations precluded the danger of injuriously high intensities of illumination under ordinary conditions.

The last twenty years, however, have brought about a complete revolution in the field of artificial illumination. The utilization of electrical energy for lighting purposes in the various forms in which it is now employed, and the remarkable development of the incandes-

cent mantle for use with gas and oil vapors have made it possible to obtain intensities of illumination scarcely dreamed of before.

Along with the increase in intensity secured by the use of the improved types of lamps have come also wide variations in the hues of the lights in common use. The carbon filament incandescent lamp is the only one of the new types whose color tone approximates that of the old time gas flame. The variations extend from the brilliant white of the Welsbach mantle and the tungsten filament lamp to the strongly colored light of such sources as the mercury vapor arc and the flaming arc, in which certain parts of the spectrum decidedly predominate, and other parts may be entirely lacking.

Further than this, the high intrinsic brightness of these recently developed types makes it possible to vary the hue at pleasure by the use of colored shades, which, notwithstanding their high absorptive indices, nevertheless transmit sufficient light to serve for ordinary purposes.

In view of the great range of variation in intensity and hue which has thus been placed at the disposal of the illuminating engineer, the question of the efficiency of the different types of lamp becomes a very important one. While it is unquestionably desirable to have higher intensities of illumination than those afforded by the gas flame, investigation has shown that increase in intensity is secured only at the cost of a greatly disproportionate expenditure of energy. There must therefore be some point beyond which it would not be expedient, from considerations of economy, to increase the illumination, and this point may best be determined by a study of the actual needs of the eye, with respect to both intensity and hue.

Economical considerations, however, do not afford the strongest argument for adapting the illumination to the needs of the human eye. So large a proportion of the world's work, especially in the large cities, must be done with the aid of artificial illumination, that the welfare of the worker becomes in reality the question of paramount importance. The rapid increase in the number of cases of defective vision has been held to be directly chargeable to the introduction of illuminants of high intensities, and it is, therefore, the first duty of the illuminating engineer to see to it that the eye-sight of those who must work under the light which he provides is properly conserved. In comparison with this consideration, economical efficiency and artistic effect become of secondary importance.

In view of these facts it is obvious that the question of visual acuity under lights of different intensities and of different colors has great practical significance, for it may be assumed as axiomatic that that light is best adapted to the eye which enables it to secure its

maximum efficiency. If it can be shown that there is a point in intensity of illumination below which the eye must work at distinct disadvantage, and beyond which an increase is not attended with a proportionate improvement in acuity, then a strict observance of this limit will be required not only for reasons of economy, but by physiological considerations as well.

Further, it is a matter of common experience that illuminations of certain hues apparently enable the eye to perceive details with greater ease than do illuminations of other hues. Whether this difference is inherent in the color, or whether it arises merely from difference in the intensities of illumination has not been established with any degree of certainty. The assumption has been very generally made and apparently accepted without question that the relation existing between visual acuity and intensity of illumination is a constant one, regardless of the color of the light. In fact, acuity has been made the basis of the judgment of intensity in a number of important physical investigations in light. Thus Lepinay and Nicati[1] determined the luminosity curve for the different portions of the spectrum by means of the acuity test, and the same method was adopted by Langley,[2] A. König,[3] and Pflüger[4] in the determination of the relation between energy and luminosity in different parts of the spectrum. Ferry[5] employed the same principle in his study of the distribution of luminosity in the light of a 16 candle-power incandescent lamp, and in his experiments on the persistence of retinal impressions. There is strong reason to believe, however, that this assumption is not accurate, and that there are specific differences in the effects of lights of different colors upon the eye, apart from those depending upon intensity.

It is the purpose of this investigation, therefore, to make a study of illumination from the point of view of visual efficiency, or adaptation to the needs of the eye, involving not only visual acuity under different degrees of illumination with so-called white light, but also the comparative acuity with lights of different colors.

In reviewing the literature of visual acuity one is struck with the wide variations in the conclusions reached by even the most careful observers. These discrepancies are traceable to two fundamental causes, namely, the failure to standardize the color values and luminous intensities of the sources used, and the lack of uniformity in the objective tests employed in determining the acuity. Discussion

[1] *Annales de Chimie et de Physique*, 5th ser., **24**, 30.

[2] *Am. Jour. Sci.*, 1888, **36**, 359–380.

[3] *Zeitschr. f. Psych. u. Physiol. d. Sinnesorgane*, 1893, **4**, 241.

[4] *Ann. d. Physik*, 1902, **9**, 185.

[5] *Am. Jour. Sci.*, 1892, **44**, 192.

of the first point will be reserved until later, when the whole question of color photometry will be treated at considerable length.

With respect to the question as to what shall constitute a satisfactory test of visual acuity the greatest variety of opinion is found. In fact, the term visual acuity itself has received several different and entirely inconsistent definitions. Some investigators have understood it to mean the ability to make fine distinctions of light and shade, while others have measured it by the power to distinguish fine details, such as small print, checker board designs, spaces between lines, etc. It is clear that two quite distinct functions of vision, the perception of light and the perception of form, are involved in these different processes, and it is to be expected that results based on the adoption of either definition would diverge greatly from those based on the other.

There is, however, fairly general agreement that the perception of detail affords the most accurate criterion of visual acuity, but even among those who unite on the main proposition there exists great difference of opinion as to the exact form which such tests should take.

Snellen has proposed the use of letters of different sizes which are standardized on the basis of a constant relation between the height of the letters and the distance at which they are to be read. That is, the distance at which any given line of type is to be read is such that it will be viewed under an angle of five minutes of arc. As far as possible, all lines and spaces in a given letter are exactly one fifth the height of the letter itself. With the Snellen test-type the formula for acuity is $V = d/D$, where d represents the actual reading distance for the tested eye, and D represents the prescribed distance at which the line should be read. This form of test-types has met practical requirements more satisfactorily than any other form yet devised, and is in very general use among optometrists.

A new form of acuity test has recently been proposed by Ives,[6] consisting of two gratings such as are used in photo-engraving, superposed upon each other. The principle involved is that if two gratings, consisting of glass plates ruled with fine parallel lines, too close to be separated by the eye (in this case 240 lines to the inch), are laid one over the other and rotated, parallel dark bands are produced, whose separation varies with the angle which the grating lines make with each other.

For this acuity target Ives claims the advantage that the details of the test object are continuously variable in size, while the illumination, the flux of light entering the eye, the distance of the object

[6] *Electrical World,* April 14, 1910, No. 55, p. 939.

and the observer's accommodation remain constant. The screen is illuminated by transmitted light.

In the selection of a satisfactory test of acuity the following requirements should be kept in view:

It should be capable of exact physical measurement, so as to be easily duplicated.

Confusing physiological effects should be eliminated, such as contrast, after-images, irradiation, state of adaptation of the eye.

Psychological sources of error should be guarded against, as suggestion, familiarity, guessing, etc.

The test object should avoid the extremes of both simplicity and complexity. If it is too simple, individual differences or unsuspected sources of error are likely to produce wide deviations in the observations. For example, if the object should consist merely of a straight line of short length, whose direction is to be determined, slight astigmatism in the eyes of the observers would give a constant advantage to certain positions of the line. On the other hand, if the object contain too many or too complex elements, there is the likelihood that the basis of judgment will vary with different observers.

There should be carefully observed the distinction between test objects that are viewed by reflected light, and those that are viewed by transmitted light. In objects that are viewed by transmitted light, there is a high probability that the light sense, as distinguished from the form sense, is playing the more prominent role. In investigations subsequently referred to, it will be found that as a rule those which show a higher acuity for the green end of the spectrum have used transmitted illumination, while those which favor the red have used reflected illumination.

The tests involving the comparative acuity with different colors and intensities should be so arranged that the comparisons need not be made directly. Each test should, if possible, be reduced to an absolute standard. Thus in the work of Dr. Bell subsequently referred to (page 38) the test characters were on two intersecting planes of a wedge, and those illuminated with green light were viewed immediately after those illuminated with white light, and the *degree* of legibility formed the basis of judgment. After the threshold of legibility has been passed, the judgment of degree of legibility becomes a very precarious one.

II

REVIEW OF PREVIOUS WORK IN VISUAL ACUITY

According to Nagel,[1] the first careful study of the relation of acuity to intensity of illumination was made by Tobias Mayer, in the year 1754. His method of observation is not given, but as a result of his investigations he formulated the law that acuity varies as the sixth root of the intensity of illumination.

In 1871, more than a century later, Cohn carried on a series of tests with untrained subjects in ordinary daylight, varying the intensity of the illumination by means of the Weber Polarisation-episkotister. Comparing his own results with those of Mayer, Posch, Albertotti, Sous and Carp, Cohn asserted as his conclusion that "enormous individual differences in visual acuity are found with the decrease in the intensity of illumination, and we are yet far from the formulation of a law for their correlation."[2]

Cohn makes the remarkable statement that he found some eyes that had unit acuity with an illumination of only 1.5 meter-candles, and half acuity with only .6 of a meter-candle. Of all eyes tested, full acuity was attained on the average at 16 meter-candles and half acuity at 4 meter-candles.

Cohn found that it was practically useless to make observations with daylight as the source of illumination, inasmuch as the eye does not by any means show the differences which the photometer shows. That is, within certain limits great variations in the intensity of the illumination are not attended with any noticeable differences in the acuity. Thus for an acuity of 1 the intensity varied to as much as ten times the minimum value; for an acuity of .75 the intensity varied to 12 times the minimum value; and for an acuity of .5 it varied to 7 times the minimum.

Dissatisfied with the enormous variations which he found, Cohn repeated his observations with a great number of persons and took the average of all the readings thus obtained. Assuming unit acuity for 100 units of intensity, an acuity of .75 was obtained with 71 units of intensity, and an acuity of .50 with 33 units of intensity.

In 1876 Posch[3] asserted as the conclusion of a series of observa-

[1] "Handbuch der Physiol. des Menschen," III., 342.
[2] *Archiv für Ophthalmologie*, 1871, **17**, (2), 305.
[3] *Archiv für Augenheilkunde*, 1876, **5**, (1), 14.

tions that the acuity increases approximately with the logarithm of the intensity; that is, the acuity increases in arithmetical progression as the intensity increases in geometrical progression. The application of this law is subject to the proviso that the intensity be not increased in a ratio greater than that of 1 to 16.

By far the most elaborate and exhaustive investigation of the relation of acuity to intensity of illumination was made by Uhthoff[4] in 1886. Experiments of the same character were repeated by König in 1897.

Uhthoff made his observations at night in a large hall, using as his source of illumination a 4 candle-power petroleum lamp, placed in a suitable box. The light was projected on the chart through a short tube, before which were placed the glass plates and liquids used in producing the variations in color. The intensity was varied in the ratio of 1:3,600,000 by varying the distance of the lamp and by the interposition of smoked glasses, whose coefficients of absorption were photometrically determined. The acuity was judged by ability to perceive the character designed by Snellen, similar in form to the letter E. This figure was cut out of black cardboard and pinned through its center to a background of white cardboard. Before each observation the character was rotated, and the subject, starting from a point too far distant for perception, slowly approached the chart until he was able to tell in which direction the two parallel lines of the figure pointed.

In the earlier course of the investigations red illumination was obtained by passing light through red glass, and blue light by passing it through an absorption cell containing a solution of copper sulphate oxidized with ammonia. This method did not produce satisfactory results, probably because of the great reduction in the luminosity resulting from the use of the color screens, and at the suggestion of König the device was adopted of covering large square tablets of white linden wood with colored fabrics. Red, yellow, green and blue cloths were obtained, which showed practically monochromatic spectra when tested with the spectroscope. The test character was pinned over these fabrics and it was then illuminated by the lamp without the intervention of screens.

Uhthoff tested chiefly persons with good acuity and color-sense, who understood fatigue of the retina, after-images, etc.

For his own eyes, Uhthoff found that unit acuity was attained at about 33 meter-candles. Another observer, *R*, reached it with slightly higher intensity. From this point the curve runs practically parallel with the axis of abscissas.

[4] *Archiv für Ophthalmologie*, 1886, **32**, (1), 171.

The following are the values obtained by Uhthoff and another observer:

	Uhthoff					"R"				
Intens.	Wh.	Yel.	Red	Gr.	Bl.	Wh.	Yel.	Red	Gr.	Bl.
3,600	2.03	2.00	1.82	.63	.45	2.00	2.15	2.00	.66	.37
1,175	1.70	1.85	1.44	.61	.39	2.00	2.15	1.74	.56	.32
400	1.52	1.69	1.33	.37	.28	1.80	2.10	1.53	.35	.25
144	1.34	1.54	1.08	.20	.17	1.58	1.68	1.12	.16	.14
36	1.05	1.13	.58	.12	.087	1.14	.92	.61	.09	.077
15	.85	.77	.43	.08	.067	.93	.74	.43	.077	.066
6	.68	.61	.26	.075	.057	.74	.53	.26	.069	.056
1.5	.33	.28	.06	.069	.046	.34	.26	.058	.058	.046
.6	.15	.18	.006	.038	.033	.21	.16	.007	.044	.033
.1	.07	.049		.004	.002	.074	.038		.004	.002
.01	.043	.018				.024	.015			

As a unit of intensity a standard candle was used at a distance of 6 meters. The following curve is plotted from Uhthoff's values, the abscissas representing the intensity in meter-candles, and the ordinates the relative acuity.

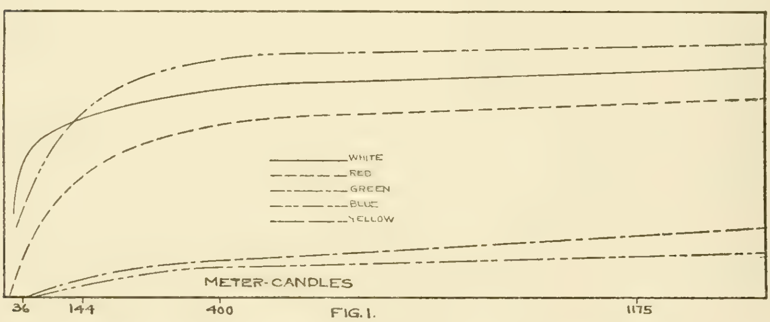


FIG. 1.

As to the relative acuity with the different colors Uhthoff found that above an intensity of about 36 meter-candles yellow gave a higher acuity than white light, but for lower intensities there was no appreciable difference. Red came next in efficiency, falling but little below the white, whereas green and blue gave values far below the other colors, ranging from about 1/3 of the acuity given by white in the higher orders of intensity to 1/10 in the lower orders.

In analyzing the method used and the results obtained in this investigation, the following possible sources of error are pointed out:

1. The character E which was used does not seem to constitute a satisfactory test object. The fact that but a single character was used throughout the observations, allowing the subject soon to become familiar with every detail of this figure, makes it possible and indeed highly probable that in many instances good guessing was

substituted for clear perception. In order to determine this point experimentally a test was made by the writer with several unexperienced observers, using Landolt's ring-form test figure, about 5 mm. in diameter with an opening of about 1 mm. The subjects were asked to indicate the fact as soon as they had approached near enough to the chart to make a good guess as to the angle in which the opening was located. They were then asked to approach until they were quite certain as to the position of the opening. It was found that in a large proportion of cases correct guesses could be made when the observers were at distances two and even three times as far as was required for clear perception. Very often the first glance would give a clue to the location of the opening, whereas prolonged gazing at the same distance would result in utter uncertainty. It is true, of course, that the practised observer may check this tendency toward guessing by setting for himself a standard of clarity of perception to be attained in each observation, but none the less the wide limit of uncertainty seems to introduce an excessive amount of inference into the method.

2. To put the black character on the colored background and then to view it with white light must necessarily produce an entirely different effect from that which would be produced by illuminating a white background containing the superimposed character with the corresponding color of light. The contrast between the black character and the blue or red fabric is much less than that between the character and the white background illuminated with blue or red light.

3. Further, it is very much to be questioned whether the photometric method used to determine the intensity of the colored lights was sufficiently accurate to insure the elimination of serious error. Many observers claim an even greater acuity for the green-blue end of the spectrum than for the red end, and my own results would tend to show that the difference is not nearly so great as Uhthoff finds to exist.

Asserting that the investigations of Uhthoff had been unsatisfactory in their results for the reason that the intensities of illumination had not been varied within sufficiently wide limits, and for the additional reason that the observations had been too few to eliminate the errors necessarily associated with the work, König[5] repeated the experiments in 1897 in a somewhat modified form. He used the same test character, the Snellen E, and adopted the same method of securing variations in color, except that he passed his light

[5] *Sitzungsbe. d. Berliner Akad. d. Wiss.*, 1897, **13**, 559.

through glass of the same color as that which forms the background of his test character. The intensity was varied by changing the distance of the source and by using different sources—the candle, large and small petroleum lamps, the Auer light and the electric arc. With white light the lower intensities were obtained by the interposition of ground glass screens. The standard of intensity was the Hefner lamp at 1 meter's distance.

Unit acuity was taken as the acuity necessary to determine with approximate certainty the position of the opening in the figure when it was viewed under an angle of five minutes. The determinations were made in three different ways:

1. The intensity of illumination was fixed, and the readings were made by the observers both receding and approaching.

2. The value of the acuity was fixed by placing the subject at a given distance, and the intensity necessary for perception at this distance was determined both by increasing and decreasing the illumination.

3. The distance of the observer and of the light were varied simultaneously until the desired clearness of perception was secured.

König states that in these observations one must be satisfied with only approximate certainty, but must be careful at the same time to maintain the same standard throughout the observations. If one demands too high a degree of certainty the eye will become fatigued within fifteen minutes, whereas if one is satisfied with less accuracy, the observations may be continued for hours without making any appreciable difference in the readings.

White		*Red*		*Green*		*Blue*	
Intens.	Acuity	Intens.	Acuity	Intens.	Acuity	Intens.	Acuity
.12	.18	.24	.07	.15	.05	.22	.02
.24	.269	.50	.10	1.00	.09	1.44	.04
.50	.46	1.15	.23	4.00	.10	2.04	.05
1.00	.596	4.94	.41	11	.11	6.02	.06
3.95	.769	8.17	.51	52	.19	26.3	.10
9.97	1.11	10	.55	112	.27	29	.08
12.88	.86	20	.67	208	.62	100	.11
20.6	1.10	44.4	.82	515	.516	260	.15
26	1.09	64	.89	2,269	1.00	629,800	.64
51	1.43	82	.87	130,200	1.63		
54.5	1.16	82	.87	130,200	1.63		
51	1.43		.92				
54.5	1.16	82	.92				
80	1.31	100	1.08				
123	1.43	205	1.11				
168	1.47	7,900	1.81				
264	1.56	40,820	1.74				

König gives the above results for his own eyes. No attempt is made to reduce the intensities for different colors to the same basis.

From these observations König deduced the following formula to express the relation between acuity of vision and the intensity of illumination:

$$S = a(\log B - \log C)$$

S denotes the visual acuity and B the intensity of illumination. The factor a is not explained, but the statement is made that it is independent of the nature of the lamp. C is a constant inversely proportional to the brightness value of the lamp used. a and C may be essentially different, according as rods or cones serve to receive the light stimulus. In the first case a may be ten times as great as in the second case.

Basing his opinion on the fact that in the low orders of illumination the acuity rises slowly with the intensity and more rapidly in the higher orders, König conjectured that in acuity two different sorts of elements of the sensitive layer in the retina are involved, namely, the rods and the cones. With the lower intensities the rods are active, and as the intensity increases they are relieved by the cones even before they reach the upper limits of their capacity. These cones likewise increase in their power until they also finally reach their limit.

König states that as a matter of fact in the determinations which belong to the slowly ascending portion of the acuity curve, fixation was not made with the fovea, but somewhat eccentrically; with the increasing intensity of illumination, corresponding to the more rapidly ascending portion of the acuity curve, fixation is made with the fovea. With a totally color-blind eye which König tested, the whole course of the curve corresponded to the slowly-ascending portion of the curve of the color-perceiving eye, and he concluded from this fact that the incapacity of the cones of such an eye is also the cause of the low acuity constantly associated with this anomaly.

With respect to the relative acuity for lights of different colors König is inclined to agree with Helmholtz in the opinion that we can, independently of color, see equally well with equal intensities of illumination.

This conclusion is controverted by Örum,[6] who studied the comparative acuity with different colors by means of groups of illuminated points on a black background. The distance at which these

[6] *Skandinavisches Archiv für Physiol.*, 1904, **16**.

points could be individually perceived was taken as the measure of acuity. Örum found that the acuity for white illumination was greater than for any one of the three fundamental colors assumed by the Young-Helmholtz theory of color vision, and that the relative acuity for the colors red, green and blue stood in the ratio of 3 : 2.5 : 2, in the order named.

At the suggestion of Nagel, who considers the work of both König and Örum as open to criticism on certain points, the problem has recently been studied by A. Boltunow.[7] Determinations of intensity were made with a Schmidt and Haensch flicker photometer. Snellen's Haken was first used as a test object, but later this was replaced with the ring-form character of Landolt. This figure was cut out of a metal plate and fixed before a square of colored glass, which was illuminated from behind by a source of light enclosed in a box. The observer thus looked directly toward the source of light, seeing only the light transmitted through the outline of the figure. The ring was rotated before each observation, and its opening was made to lie in one of four positions, above, below, or to the right or left side.

The observer indicated the position of the opening ten times for each distance chosen, and when he was correct eight times out of the ten, this distance was accepted as the proper one.

Only one degree of intensity was used, and this was not described in standard units. It was only known that the illuminations given by the different lights were of the same intensity, as determined by photometric comparison. At first a very high intensity was used, so that the optimum acuity was exceeded. The illumination was consequently reduced one half by means of an episkotister, when it was found that actual increase of acuity resulted. This fact in itself indicates that there was some serious defect in the method. It is conceivable that a further reduction of intensity might have resulted in still higher acuity, and we should thus have the acuity varying inversely as the intensity, which is of course contrary to all normal experience.

Boltunow concludes from his observations that white light affords the highest acuity, with green next in order, and that red is the least efficient. So far as high intensities are concerned, he attributes the difference between red and green to various physical factors, as aberration, irradiation, etc.

As was pointed out in the case of König's work, the test character employed in this investigation does not seem well adapted to securing

[7] "Über die Sehschärfe im farbigen Licht," *Zeitschrift f. Psych. u. Physiol. d. Sinnesorgane*, 1907–8, **42**, (2), 359.

accurate measures of acuity. The wide range of values secured by the same observer gives evidence of this. For the red illumination the distance representing the measure of acuity ranged from 430 cm. to 520 cm., and for the green it ranged from 510 cm. to 610 cm.

The last recent study of visual acuity as related to illumination of different colors to which reference will here be made was undertaken by Messrs. Broca and Laporte[8] in Paris in 1908. They employed sources of light in ordinary commercial use, the carbon filament incandescent lamp, the ordinary carbon arc lamp, and the mercury vapor arc. The effects of these lights upon the eye were studied with respect to acuity, speed of reading, and fatigue.

Figure 2 represents a diagram of the apparatus which was used for determining the intensity of illumination, and for making the various tests with the eye.

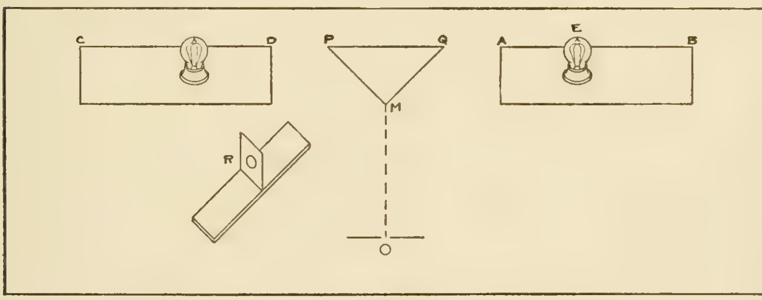


FIG. 2.

A Ritchie wedge, 45 degrees, *PMQ* in the figure, is placed at one end of a photometric bench, *AB*, so that the plane which bisects the angle *PMQ* of the prism is perpendicular to the axis of the bench. On the surface of the prism *PM* was placed a Parinaud's optometric chart, which was illuminated by the source of light under observation. The opposite plane of the prism carried a piece of paper of the same whiteness as the chart, but without printing. This plane was illuminated by a standard lamp on the photometric bench. In the bisecting plane of the prism, at *O*, was a screen at which the observer placed his eye in order to determine the relative intensity of illumination of the two surfaces of the prism. This determination was made by the ordinary direct method, the two surfaces being assumed to be equally illuminated when they seemed to be so to the observer. It is admitted by the authors that this photometer lacks sensitiveness, and that different observers obtained systematic differences in

[8] *Bulletin de la Société Internationale des Electriciens,* Paris, 1908, 2d series, **8**, No. 76.

their determinations. Each observer made his acuity tests on the basis of his own photometric values. In view of the great difficulty involved in making direct comparison of colored lights pointed out elsewhere, it would seem that this failure to determine with greater exactness the relative intensities of the different lights would greatly impair the value of these observations.

Perpendicular to the plane of the optometric chart was a bench carrying a screen *R*, with an aperture of 2 mm. in diameter, through which the chart was read in order to determine the acuity. This screen served the double purpose of fixing the position of the eye at its maximum reading distance, and of eliminating variations due to changes in the size of the pupil. Inasmuch as the diameter of the pupil would vary under these illuminations from 4 to possibly 7 mm., this arrangement would result in cutting down the area of the pupil exposed to the lower intensities to a value of only one fourth to one twelfth of its actual size. Certainly this would produce a great deviation from actual reading conditions, and the authors state that the acuity was in fact considerably diminished by this screen for low illuminations.

The conclusion is drawn from these observations that ''all ordinary sources of light, when they give to a paper the same degree of luminosity, give to the observer at the same time the same visual acuity and speed of reading. It is almost certain in such cases that the retinal fatigue is the same, and that the contraction of the pupil is also the same.''

III

PRELIMINARY STUDY

The preliminary observations here reported were made not so much with the hope of securing accurate results as with the purpose of becoming familiar by actual trial with the important details of procedure, in order that sources of error might be discovered and, as far as possible, eliminated before taking up the more exact study of the problem.

In making choice of the various tests proposed for the study of visual acuity, the writer was governed largely by the consideration that the chief function which the eye is called upon to perform under artificial illumination is that of reading. Characters of the alphabet were therefore chosen. Even though such a test-object may be open to theoretical objections, its use would seem to yield results of the highest degree of practical application, in view of the very general use of such characters among optometrists.

The apparatus and method of procedure were as follows: At one end of a long bench conveniently graduated was placed a stand supporting a Snellen's optometric chart. On this chart the line of type was selected which should be read by the normal eye under average daylight illumination at a distance of three meters. This line was chosen for the reason that it happened to be the most convenient for the distances and intensities of illumination available. It was the intention at first to use all the letters in the line for observation, but a few trials developed the fact that the letters varied considerably in legibility, and full perception of the line therefore resolved itself into perception of the least legible letter in the line. This letter was found to be *R*, and consequently the attention was centered upon it, the effort being made to perceive it at each observation with the same degree of distinctness.

For the illumination of the chart the following sources of light were used:

Ordinary carbon filament lamps, with clear glass bulbs, with nominal ratings of 2 and 16 candle power respectively;

Carbon filament incandescent lamp, with ruby bulb, nominally rated at 16 candle power, but with an actual intensity, as subsequently determined photometrically, of .6 candle power;

Carbon filament incandescent lamp, with blue bulb, rated at 16 candle power, with an actual intensity of 1.1 candle power;

Carbon filament incandescent lamp, with green bulb, rated at 16 candle power, with an actual intensity of 1.6 candle power.

Spectroscopic tests of these colored bulbs showed that the red lamp yielded practically monochromatic light, while the green and blue bulbs transmitted light of all wave-lengths.

In order to get higher intensities in the red light, a second and larger lamp of the same type was used. Although its nominal rating was twice that of the smaller lamp, it was necessary to operate it at a much lower voltage, so that it yielded a calculated candle power of only .76. Readings with this lamp were overlapped with those of the smaller one, and the values were found to be approximately identical. These lamps were all supplied with current from a storage battery circuit, and care was taken that the voltage remain constant during the observations.

The method of determining the candle power of the colored lamps will be fully discussed in a later section.

The intensity of illumination with any given lamp was varied by varying its distance from the chart, the intensity being calculated by the law of inverse squares. The illuminating lamp was placed in a small wooden box, so arranged that its light could fall upon the chart, but could not in any way directly affect the eye of the observer. The walls, floor, ceiling and furnishings of the room in which the experiments were conducted were painted black, so that the effect of diffused light may be regarded as negligible. Sufficient time was always allowed for adaptation in going from daylight into the darkroom.

The illumination of the chart having been suitably arranged, the observer would recede to a point at which the letters were entirely illegible, and then gradually approach the chart, stopping at the point at which he secured the desired clearness of perception of the test-character. This distance was carefully measured with a flexible metric scale, which extended from the chart to the eye of the observer. Each observation was repeated at least five times, and the average of all the readings was then calculated. The greatest deviation from the average distance was usually within 5 per cent. of the total distance, although it was found difficult to decide for one's self, under all conditions, what degree of legibility was required. Thus for high intensities of illumination one would feel that he was instinctively demanding a clearer perception than when the intensity was low. Efforts were of course made to reduce such effects to a minimum.

After determining the average reading distance for any given illumination, the visual acuity was calculated by dividing the found

Acuity Readings

TABLE I

With 2 cp. Carbon Filament Lamp

Intensity in Meter-candles	Acuity	Reading Dist. in cm. Av.	Range
.125	.29	88	85– 92
.25	.40	120	110–130
.50	.49	148	134–162
1	.57	171	160–182
2	.69	206	200–210
4	.77	231	225–240
8	.85	255	245–265
12	.89	269	260–275
16	.95	285	270–295
20	.97	291	290–295
24	1.00	301	295–305
28	1.02	305	300–310
32	1.04	313	310–325
Supplementary with 16 cp. lamp			
4	.76	228	227–230
8	.85	256	252–260
16	.94	281	270–288
32	1.02	305	300–310
64	1.05	316	313–320
128	1.07	322	318–325
256	1.10	330	325–332

TABLE II

With Red Bulb

Intensity in Meter-candles	Acuity	Reading Dist. in cm. Av.	Range
.03	.05	15	15– 16
.036	.12	36	35– 37
.05	.15	46	44– 48
.06	.19	58	55– 58
.10	.22	67	64– 70
.15	.31	92	90– 94
.26	.36	109	107–110
.60	.47	140	138–145
1.22	.63	191	188–195
1.67	.64	193	188–200
2.40	.68	205	195–212
2.96	.73	218	215–225
3.74	.74	223	215–230
4.90	.79	238	228–248
6.67	.82	245	242–248
9.60	.87	260	250–265
15.00	.92	275	270–280
Supplementary with .767 cp. lamp			
12	.91	274	270–280
16	.96	289	280–295
20	1.01	303	300–310

TABLE III

With Green Bulb

Intensity in Meter-candles	Acuity	Reading Dist. in cm. Av.	Range
.08	.20	62	60– 65
.10	.23	70	70– 70
.13	.26	79	75– 82
.18	.31	94	90–100
.26	.35	105	100–108
.40	.42	127	125–130
.71	.49	148	145–150
1.60	.63	190	187–192
3.26	.69	207	204–210
4.44	.72	215	210–220
6.40	.75	224	220–230
7.90	.82	245	240–250
13.06	.87	260	250–275
17.77	.91	274	270–280
25.60	.95	285	280–290
40	1.04	312	300–320

TABLE IV

With Blue Bulb

Intensity in Meter-candles	Acuity	Reading Dist. in cm. Av.	Range
.05	.11	34	33– 34
.06	.15	44	40– 47
.08	.20	60	58– 62
.11	.22	67	65– 69
.18	.26	77	77– 77
.26	.30	90	87– 92
.48	.36	108	105–110
1.10	.52	155	150–160
2.25	.60	182	175–188
3.05	.64	192	188–198
4.40	.69	207	200–210
5.10	.72	216	212–220
6.90	.74	223	218–226
8.98	.80	241	235–245
12.21	.87	262	258–265
17.60	.88	264	260–270
27.50	.92	275	270–280

distance in centimeters by 300, the standard distance of the line. Thus an illumination which allowed the line to be clearly perceived at 150 centimeters was said to give an acuity of .5.

The writer determined by test that his own eyes, aided by spectacles, possessed practically unit acuity under average daylight illumination.

Tables I, II, III and IV give the results of these observations.

Table V. gives a summary of the acuity values obtained with the lights of different colors, arranged with a view to easy comparison:

TABLE V

ACUITY AND INTENSITY.—SUMMARY

Intens. in M.-c.	Wh.	Red	Gr.	Bl.	Intens. in M.-c.	Wh.	Red	Gr.	Bl.
.03		.05			4.00	.76 .77			
.036		.12			4.40				.69
.05		.15		.11	4.44			.72	
.06		.19		.15	4.90		.79		
.08			.20	.20	5.10				.72
.10		.22	.23		6.40			.75	
.11				.22	6.67		.82		
.125	.29				6.90				.74
.13			.26		7.90			.82	
.15		.31			8.00	.85 .85			
.18			.31	.26	8.98				.80
.25	.40				9.60		.87		
.26		.36	.35	.30	12.00	.89	.91		
.40			.42		12.21				.87
.48				.36	13.06			.87	
.50	.49				15.00		.92		
.60		.47			16.00	.95 .94	.96		
.71			.49		17.60				.88
1.00	.57				17.77			.91	
1.10				.52	20.00	.97	1.01		
1.22		.63			24.00	1.00			
1.60			.63		25.60			.95	
1.67		.64			27.50				.92
2.00	.69				28.00	1.02			
2.25				.60	32.00	1.04 1.02			
2.40		.68			40.00			1.04	
2.96		.73			64.00	1.05			
3.05				.64	128.00	1.07			
3.26			.69		256.00	1.10			
3.74		.74							

While it would be unsafe to attempt to draw any definite conclusions from these preliminary observations, the following points may be indicated as being brought out with considerable clearness:

1. The increase of acuity with intensity is very much more rapid in the lower orders of illumination.

$\frac{1}{4}$ acuity is obtained with about	.10	meter-candle intensity;
$\frac{1}{2}$ acuity is obtained with about	.60	meter-candle intensity;
$\frac{3}{4}$ acuity is obtained with about	4.00	meter-candles intensity;
unit acuity is obtained with about	24.00	meter-candles intensity;
1.10 acuity is obtained with about	256.00	meter-candles intensity.

2. The curve of acuity (Fig. 3) which is plotted from the figures given, using the measures of intensity in meter-candles as abscissas and the measures of acuity as ordinates, is seen already to have turned from a vertical toward a horizontal direction at about 2 meter-candles intensity; after unit acuity is reached the curve becomes approximately parallel to the axis of abscissas.

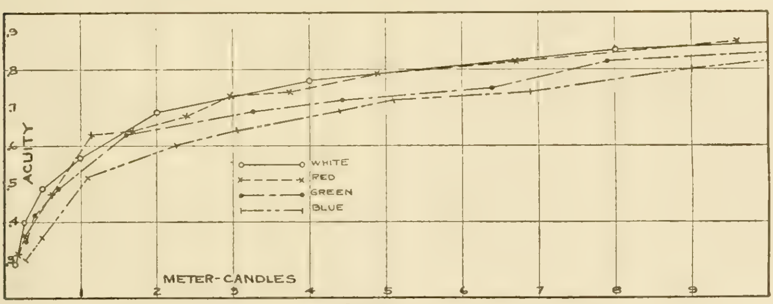


FIG. 3.

3. As to the effect of different colors upon acuity, while the writer does not find nearly so great a decrease in efficiency for the green and blue lights as Uhthoff found, it is quite clear that whatever differences may exist are in favor of the colors at the red end of the spectrum.

This difference between the detail-revealing power of red light and that of blue or green light is strikingly brought out by ordinary direct observation in the dark room. Under illumination with the red lamp the walls and tables, which were painted black, at some distance from the lamp were shrouded in gloom, while with the green light the walls and other objects were quite conspicuous. When, however, the test chart was exposed, the black characters stood out clearly defined against the white background under the red illumination, whereas with the green light the characters were scarcely perceptible. This difference is apparently due to the fact that the black

absorbs practically all the red rays which fall upon it, whereas a large proportion of the green light is reflected by it.

Improvements in Method

In the course of these preliminary observations several possible sources of considerable error were discovered. The first of these has already been alluded to, and consists in the large element of guessing which is involved in observations with a test-character which is, or may soon become, perfectly familiar. In order to eliminate this element as far as possible and yet retain a test of the same general character, for the Snellen's chart was substituted a series of letters of heavy faced Gothic type of approximately the size of the Snellen 3-meter line. The following line shows the exact size and style of the type used:

A B C D E F G H

Those letters of the alphabet were discarded which seemed especially legible, as the I and L. Several numerals were used also. Most of the characters were used three times, giving a total of about 80 characters in the whole series. These were arranged along the circumference of a cardboard disc about 8 inches in diameter in such order as to avoid the recurrence of similar combinations of letters. This disc was mounted vertically on an axis, and was covered with a plate having near its upper side a slot through which a series of six characters could be exposed at one time. The disc was rotated for each observation, and the observer was required to approach the disc until five of the six letters could be read correctly to the assistant.

A second source of error was associated with the generally recognized difficulty involved in making photometric measurements of the intensity of lights of different colors, and at low orders of illumination. In the preliminary experiments it was necessary to carry the lamps used to the physics laboratory in order to determine their intensity photometrically. This laboratory had white walls and a considerable amount of diffused illumination which could not be eliminated. This defect, along with the difficulties involved in exactly duplicating the voltages of the lamps in the dark room and in the laboratory, introduced a considerable amount of uncertainty into the determinations of the candle power of the lamps. It was therefore found advisable to set up a photometer in the dark room, and make the acuity observations at the same time that the lamps were photometered.

The plan adopted was as follows: A 2-candle-power carbon filament incandescent lamp, with constant voltage, was set up on the right-hand side of the photometer head at a fixed distance, and the lamp which was intended to be used for the acuity tests was brought into balance on the other side. This lamp was then set at the same distance from the disc with the test-characters, and the acuity observations were made.

The 2-cp. lamp was then set at another fixed distance and the process repeated. In this manner not only the colored lamps but also a carbon filament 4-cp. lamp and a photometric standard lamp were balanced against the 2-cp. lamp, and thus conditions of illumination were duplicated for each lamp, and the absolute intensities of the lamps at the given voltage were determined as well.

The following tables give the results of these observations. The 2-cp. lamp was maintained at 112 volts, and the red, green and blue lamps at 108 volts. The standard lamp was rated at 16 candle power at 106.1 volts, and the 4-cp. lamp used in the observations was maintained at 112 volts.

TABLE VI

Dist. of 2-cp.	*Distance in Cm. to Balance of*				
Lamp in cm.	Red	Green	Blue	4-cp.	Stand.
500	*268	*434	*359		
400	*214	*347	*282		
300	*160	*260	211	*447	
250	*133	*217	176	*372	
200	*107	*173	141	*298	*550
150	*80	129	107	*223	*411
100	53.6	86.5	71.8	149	278
75	40	65.5	54	112	205.8
50	26.5	44.2	*36	74.5	137.5

* Distances marked with asterisk are estimated on basis of readings taken at 150, 100, and 75 cm., as these are more accurate, because of higher intensity.

Determination of Candle Power

The values given in Table VI. were used to determine the absolute candle power of the red, green and blue bulbs used in the preliminary experiments, for the reason that they were secured under improved photometric conditions, and may therefore be presumed to be more exact than the earlier determination made in the physical laboratory.

All readings taken at different distances of the 2-cp. lamp are in Table VII reduced to their equivalents at 100 cm. That is, readings at 50 cm. are doubled, those at 75 cm. are increased by one third, etc. A rough average is then made of these different readings,

weight being given to those values which were actually made at 100 cm. and 75 cm., rather than to the reduced values.

TABLE VII

Dist. of 2-cp. Lamp in Cm.	*Red* Actual Reading in Cm.	Reduced	*Green* Actual Reading	Reduced	*Blue* Actual Reading	Reduced	*Standard* Actual Reading	Reduced
300					211	70.3		
250					176.5	70.6		
200					141	70.5		
150			128.8	85.9	107	71.3		
100	53.6	*53.6*	86.5	*86.5*	71.8	*71.8*	278	*278*
75	40	53.3	65.5	87.3	54	72	206	275
50	26.5	53	44.2	88.4			137.5	275
	Average	*53.5*		*87*		*71.5*		*276*

The candle power of the red, green and blue lamps will have the same ratios to the standard 16 cp. as the squares of their respective distances. Thus the candle power of the red is $53.5^2/276^2 \times 16$ cp. or .60 cp.; the candle power of the green lamp is $87^2/276^2 \times 16$ cp. or 1.6 cp.; the candle power of the green lamp is $71.5^2/276^2 \times 16$ cp. or 1.1 cp.

The acuity readings made under these modified conditions are given in Table VIII. Table IX. gives the results with distances of the lamps reduced to intensity in meter-candles and the reading distances reduced to acuity, the latter value being obtained by dividing the distances by 272 cm. This divisor represents the average of 21 readings of the chart under average daylight illumination, with an average deviation of 7 cm. These readings were as follows: 258, 259, 260, 262, 264, 264, 266, 268, 270, 271, 274, 275, 275, 276, 276, 277, 280, 281, 283, 283, 288.

TABLE VIII

Reading Distances with Red, Green, Blue Standard and 4-cp. Lamps, when Balanced against 2-cp. Lamp at Fixed Distances

2-cp. Lamp, Cm.	Red, Cm.	A. D., Cm.	Green, Cm.	A. D., Cm.	Blue, Cm.	A. D., Cm.	Stand., Cm.	A. D., Cm.	4-cp. Cm.	A. D., Cm.
500	50	3	47	1						
400	62	1.5	62	1.5						
300	89	4	75	2	80	2			91	2
250	100	4	87	2	92	3			112	3
200	119	4	100	4	104	2	122	3	120	4.7
150	141	2.8	117	2	113	3	143	4	145	3
100	174	4	151	2.4	137	5	170	3	161	2
75	195	4	181	4	161	3	195	1.5	189	4.6
50	224	3	208	4	181	3	219	5	218	2.5

TABLE IX

Distance of 2-cp Lamp. Cm.	Intensity in Meter-candles	*Acuity for* Red	Green	Blue	Standard	4-cp.
500	.085	.183				
400	.133	.228	.228			
300	.236	.327	.275	.294		.334
250	.340	.367	.319	.338		.411
200	.531	.437	.367	.382	.448	.440
150	.944	.517	.429	.415	.525	.532
100	2.125	.639	.554	.503	.624	.591
75	3.778	.716	.664	.591	.716	.694
50	8.500	.822	.763	.664	.804	.800

Continued with standard lamp:

5.33	.752
12.00	.936
21.33	1.035
48.00	1.127
85.33	1.185
192.00	1.270

The curves (Fig. 4) which are plotted from these values show the same general characteristics as those obtained from the preliminary observations. The red and the white illuminations yield approximately equal acuity, while both are considerably higher than the green. The acuity with the blue illumination is the lowest.

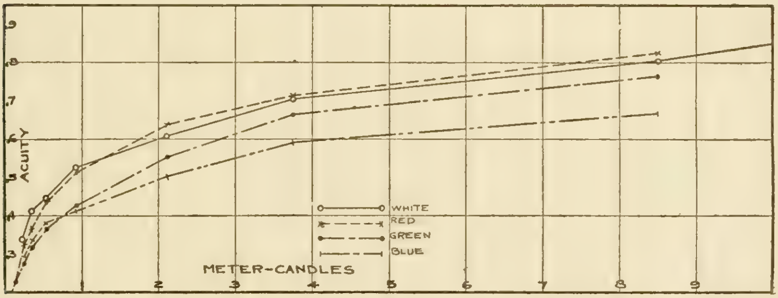


FIG.4.

IV

MAIN OBSERVATIONS IN ACUITY

Attention has already been called to the fact that of the colored incandescent bulbs used in the preceding observations only the red gave approximately monochromatic light, and this was of very low intensity. In order to increase the intensity of the red illumination and to secure more nearly pure colors in the green and the blue, it was found necessary to adopt a different source of light.

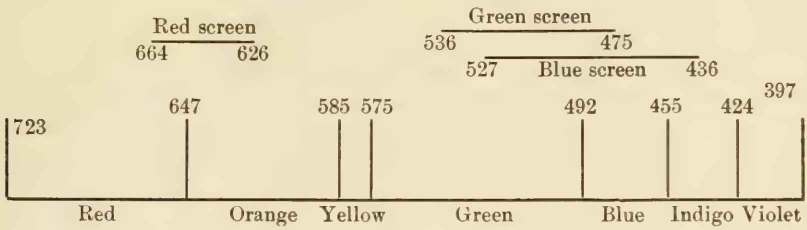
An attempt was made to project a spectrum by means of a collimating lens and prism, following the arrangement used by Abney in his work on color vision. This method, however, was found impracticable, for the reason that it was impossible to secure a sufficiently large field of uniform intensity and color tone to meet the requirements of this study.

Several other devices were tried, as combinations of colored glasses and gelatines, but none of these yielding satisfactory results, recourse was finally had to colored liquids contained in absorption cells. For red a dye was used called poncean red, a strong solution of which transmitted a color well within the limits of the red, ranging from 664 $\mu\mu$ to 626 $\mu\mu$. The red incandescent bulb and a glass plate which was subsequently used yielded light within practically the same limits of wave-length.

In order to secure a satisfactory green it was necessary to use two solutions—a saturated solution of copper chloride and a weak solution of crystal violet. This yielded an illumination in which green strongly predominated, without any yellow but with a touch of blue. The wave-length ranged from 536 $\mu\mu$ to 475 $\mu\mu$, but the blue was of so low intensity that its effect may be considered negligible.

It was found to be entirely impossible to produce a pure blue of sufficient intensity to be accurately photometered or to be used in acuity tests. A solution of copper sulphate oxidized with an excess of ammonia gave a practically pure blue, if left of full strength, but as has already been said, the intensity of the light thus transmitted was too low to be available. It was necessary therefore to dilute the solution with water, and this extended the spectrum a considerable distance into the green, the wave-length ranging from 527 $\mu\mu$ to 436 $\mu\mu$. To the direct vision, however, the illumination was distinctly blue, there being no suggestion of the green component.

The extent of the spectrum covered by the different colors is represented in the following diagram, based on Listing's scale:



As the source of light a Nernst glower was used, of 68.25 candle power, enclosed in a cubical box of about 12 inches on a side. The light was emitted through an aperture in one side of the box, about three inches in diameter, before which the cells containing the liquids were placed.

In this series of observations three persons in addition to the writer acted as observers. For each the acuity in daylight vision was determined by twenty readings of the chart, and this value was used as a basis in determining the acuity with the colored lights. The observers were cautioned not to attempt to beat previous records in successive trials, but rather to adopt a fixed standard of legibility and to adhere to it as closely as possible in each observation.

It was found that it was not always possible to keep exactly the same angle between the line of sight of the observer and the direction of the light incident upon the chart, and this change of angle was likely to be accompanied by more or less glare. Wide variations in any series of readings may find a partial explanation in this fact.

Observers would occasionally find difficulty in the clearing up of the characters, dependent not upon the intensity of illumination, but rather upon the defective focusing of the eye.

The following tables give the results for the different observers:

TABLE X

Observer Br.

White		*Red*		*Green*		*Blue*	
Intensity in M-c.	Acuity	Intensity	Acuity	Intensity	Acuity	Intensity	Acuity
.96	.47	.62	.43	.11	.18	.10	.16
1.28	.52	.97	.48	.16	.21	.18	.18
1.91	.61	1.72	.52	.29	.25	.41	.21
3.51	.66	3.90	.55	.66	.33	.73	.25
5.10	.69	6.90	.58	1.17	.39	1.64	.33
7.97	.72	15.60	.73	2.65	.43	2.56	.37
		24.37	.75	5.40	.49		

TABLE XI

OBSERVER H.

White Intensity in M-c.	Acuity	*Red* Intensity	Acuity	*Green* Intensity	Acuity	*Blue* Intensity	Acuity
1.24	.46	.09	.21	.02	.08	.02	.10
1.79	.56	.12	.26	.05	.12	.05	.14
2.79	.63	.19	.30	.12	.19	.12	.19
4.95	.74	.33	.38	.21	.25	.20	.26
11.17	.73	.75	.46	.48	.34	.46	.32
19.81	.76	1.33	.50	.85	.42	.82	.40
44.70	.83	3.00	.53	1.92	.48	1.84	.46
178.80	.92	12.00	.61	3.91	.57	3.75	.54
		24.00	.68				

TABLE XII

OBSERVER BE.

White Intensity in M-c.	Acuity	*Red* Intensity	Acuity	*Green* Intensity	Acuity	*Blue* Intensity	Acuity
1.00	.49	.125	.25	.125	.14	.125	.13
2.00	.60	.25	.40	.25	.21	.25	.19
4.00	.68	.50	.55	.50	.27	.50	.24
8.00	.77	1.00	.60	1.00	.34	1.00	.32
16.00	.84	2.00	.71	2.00	.40	2.00	.38
32.00	.91	4.00	.75	4.00	.48	4.00	.43
64.00	.95	9.40	.82	8.00	.56	8.00	.48
		16.00	.89	16.00	.59		

TABLE XIII

OBSERVER RI.

Red Intensity	Acuity	*Green* Intensity	Acuity	*Blue* Intensity	Acuity
.125	.25	.11	.18	.10	.17
		.16	.21	.18	.22
.25	.36	.29	.25	.41	.30
		.66	.36	.73	.39
.50	.48	1.17	.41	1.64	.49
		2.65	.52	2.56	.53
1.00	.56	5.40	.59		
2.00	.61				
4.00	.72				
8.00	.79				
9.40	.81				
16.00	.86				

The accompanying curves (Figs. 5, 6, 7, 8) represent the values obtained by the individual observers in graphic form. In Fig. 8 the lower of the two curves for red represents a series of values not given in the tables.

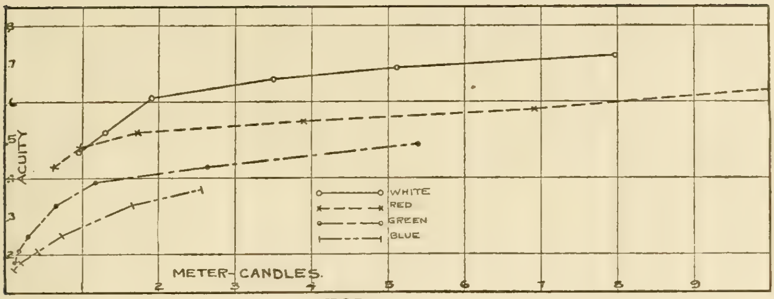


FIG.5.

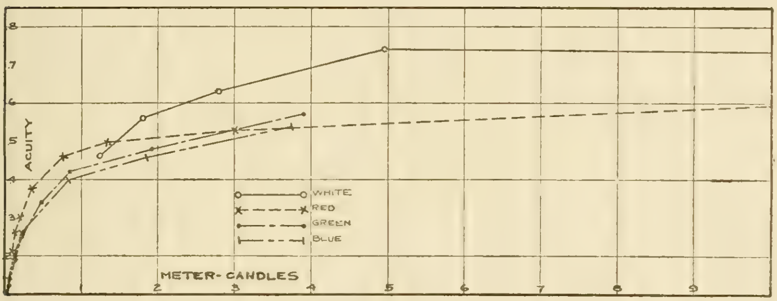


FIG.6.

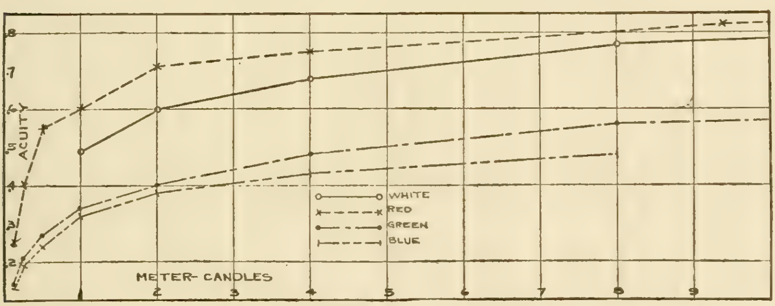


FIG.7.

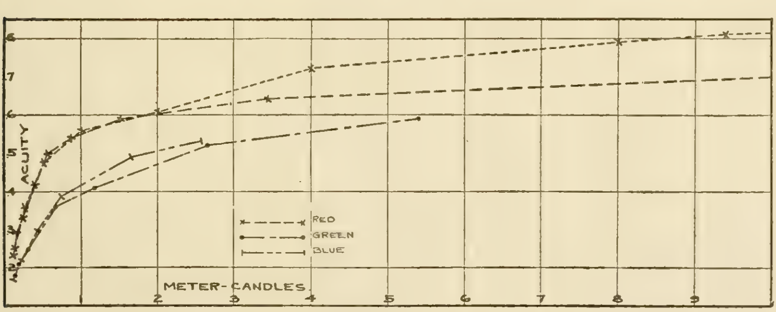


FIG.8.

In order to compare the results of each individual for the different colors and as well to secure the averages for the different observers it is convenient to reduce the values to the same series of abscissas. This was done by taking the values of the ordinates for fixed points along the axis of abscissas.

Table XIV. gives the acuity values of each observer for the various colors and for white.

TABLE XIV

Intensities in Meter-candles

	.125	.25	.50	1.	2.	3.	4.	5.	8.	10.
Ri.										
White	.29	.36	.44	.53	.60	.65	.71	.73	.79	.86
Red	.25	.36	.48	.56	.61	.67	.72	.74	.79	.81
Green	.19	.23	.31	.40	.48	.53	.55	.58		
Blue	.18	.23	.32	.42	.50	.54				
Br.										
White				.47	.62	.65	.67	.69	.72	
Red				.49	.52	.54	.56	.57	.60	.63
Green	.19	.24	.30	.37	.41	.44	.46	.48		
Blue	.17	.19	.22	.28	.34					
H.										
White					.58	.64	.69	.74	.74	.74
Red	.26	.34	.42	.48	.51	.53	.54	.55	.58	.59
Green	.19	.26	.34	.43	.49	.53	.58			
Blue	.19	.27	.32	.41	.47	.51	.55			
Be.										
White				.49	.60	.64	.68	.70	.77	.78
Red	.25	.40	.55	.60	.71	.73	.75	.76	.80	.82
Green	.14	.21	.27	.34	.40	.44	.48	.50	.56	.57
Blue	.13	.19	.24	.32	.38	.41	.43	.44	.48	

With the exception of the case of the writer, in which the values for the blue lie slightly above those for the green, it will be observed that the same order of colors is obtained by the different observers as was obtained by the writer in previous determinations, viz., red, green and blue. The following special points are of interest in connection with these tables:

1. Although the distances at which the different observers could read the test characters under daylight illumination varied considerably, their acuity values for white illumination, based on their daylight acuity, agree very closely. The reading distances for each observer with daylight illumination were as follows: Ri., 272 cm., a.d. 7 cm.; Br., 316 cm., a.d. 5 cm.; H., 310 cm., a.d. 8 cm.; Be.,

305 cm., a.d. 6 cm. The slightly higher values obtained by the writer may find a partial explanation in the fact that his determinations were made with illumination from carbon filament lamps, whereas those of the others were made with illumination from a Nernst glower, reduced in intensity by the interposition of a ground glass plate. The light from the latter source was consequently much weaker in red rays than that from the carbon filament lamps.

2. Considerable individual differences are shown with respect to the acuity with the different colors. The following table shows the relative position of the different observers, ranked roughly according to their acuity with the different colors:

	White	Red	Green	Blue
Ri.	1	2	2	1
H.	2	4	1	2
Be.	3–4	1	4	3
Br.	4–3	3	3	4

With the white and blue lights the relative positions are the same, and the individuals who hold the extremes with these colors hold mean positions with the red and green. On the other hand, H., who ranks lowest with red illumination, ranks highest with the green, and Be., who ranks highest with the red illumination, has the lowest rank with the green. These differences correspond to differences which were incidentally brought out in connection with photometric determinations of the colored lights. In determining the candle powers of the illuminations Be. secured a considerably higher value for the red than did the writer, and a somewhat lower value for the green. H., on the other hand, secured a higher value for the green than did the writer. These observations on the luminosity value of the lights were not made with sufficient accuracy to carry any great significance, but the comparison is at least suggestive.

It would seem from these observations as from others of similar character that sensitivity to the green is in an inverse relation to sensitivity to the red, a high sensitivity to red implying a low sensitivity to green, and vice versa.

TABLE XV

INTENSITIES IN METER-CANDLES

	.125	.25	.50	1.	2.	3.	4.	5.	8.	10.
White	*.29	*.36	*.44	*.50	.60	.64	.69	.71	.75	*.79
Red	*.25	*.37	*.48	.53	.59	.62	.64	.65	.69	.71
Green	.18	.23	.30	.38	.44	.48	.52	*.52	*.56	*.57
Blue	.17	.22	.27	.36	.42	.48	*.49	*.44	*.48	

Table XV. gives the *average* acuity of the four observers for different intensities of the different colors and white. In some cases this value is secured from fewer than four observers, for the reason that the observers did not all cover the total range of intensities. Such partial averages are indicated by an asterisk.

The curves plotted from these average values are shown in Fig. 9. The numeral above any point on a curve indicates the number of individual values entering into the average. In cases where the numerals are omitted, the average represents the values of all four observers.

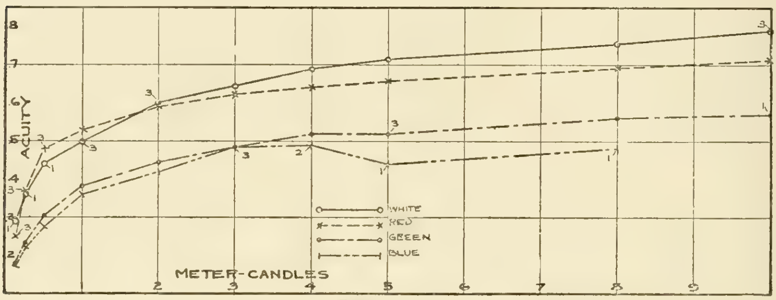


FIG. 9.

The conclusions which may be drawn from the average results of the four observers coincide with those from the preliminary observations about as closely as measurements made by one individual may be expected to coincide with an average.

Considering, first, the change in acuity as related to variations in the intensity of white light, it may be said that an acuity of one fourth is secured with an intensity of less than one eighth of a meter-candle; one half acuity with one meter-candle or less; and three fourths acuity with eight meter-candles or less of intensity. This series of observations was not run sufficiently high in intensity to attain unit acuity, although from the data secured it is obvious that a higher intensity would have been required than was required in the preliminary observations. The difference is probably to be explained by the difference in the test characters which were used.

With respect to the comparative efficiency of lights of different colors, these results make it clear that the red end of the spectrum is much more favorable to acuity than the green, the values for the red ranging from 20 to 50 per cent. above those for the green. As between red illumination and uncolored illumination, the difference appears to be very slight, although, on the whole, it is in favor of uncolored illumination. The values for blue run very close to those for green, but somewhat lower.

V

SUPPLEMENTARY OBSERVATIONS

In order to include a greater number of observers in the acuity test, the investigation, which was necessarily discontinued in June, 1910, was resumed in May, 1911. The procedure was the same as in the earlier experiments, except that a change was made in the source of the colored illumination. In the course of the work with the colored liquids it was found that these were subject to continual and irregular changes in density, due to evaporation and heating from the glower during the observations. A satisfactory substitute for the green liquid was found in the photographic color filter prepared by the Cramer Dry Plate Company, of St. Louis, Mo. This filter transmits a band in the spectrum lying between the wave-lengths of 490 and 560 $\mu\mu$.

To secure red illumination a piece of ruby glass was substituted for the poncean red solution. This yielded a pure red with approximately the same limits of wave-length as the liquid. Observations with blue illumination were discontinued, inasmuch as the work previously done indicated that so far as acuity was concerned the blue illumination might be regarded merely as a modification of the green.

In view of the fact that the general direction of the acuity curve was determined with sufficient definiteness in previous observations, it was not considered necessary in this series to use more than four variations of intensity with the colored lights, two points being located in the rapidly ascending portion of the curve, and the other two in that portion of the curve in which large increases in intensity are attended with but slight variations in acuity. For the uncolored illumination only two degrees of intensity were used, as these were sufficient to afford a satisfactory basis of comparison.

Table XVI. gives the results of these additional acuity observations for the several individuals, and Table XVII. gives the same values reduced to equal intensities, for purposes of comparison. The values given in the latter table were gotten by plotting the curves (Figs. 10, 11, 12, 13, 14) from Table XVI., and measuring the ordinates for the given abscissas, after smoothing out the curves. The figures given are, therefore, only approximate.

TABLE XVI

P.

	White		Red				Green			
Intensity .	8.46	15.42	.41	.75	3.00	18.75	.84	1.54	6.15	38.40
Acuity . . .	.68	.75	.38	.45	.60	.74	.52	.55	.64	.73

H.

	White		Red				Green			
Intensity .	8.46	15.42	.39	.71	2.83	17.70	.77	1.40	5.60	35.00
Acuity . . .	.65	.74	.35	.42	.57	.73	.38	.42	.57	.71

W.

	White		Red				Green			
Intensity .	7.68	13.75	.37	.68	2.75	17.10	.69	1.25	5.00	31.20
Acuity . . .	.72	.75	.37	.42	.56	.67	.43	.53	.65	.83

S.

	White		Red				Green			
Intensity .	8.46	15.42	.43	.79	3.15	19.81	.84	1.53	6.11	38.19
Acuity . . .	.81	.85	.51	.61	.75	.86	.41	.49	.64	.72

R.

	White		Red				Green			
Intensity .	8.46	15.42	.32	.58	2.32	14.50	.85	1.56	6.25	39.06
Acuity . . .	.79	.82	.31	.42	.64	.97	.49	.54	.65	.91

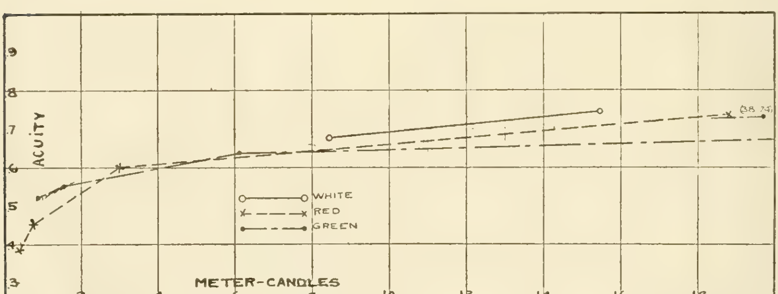


FIG 10.

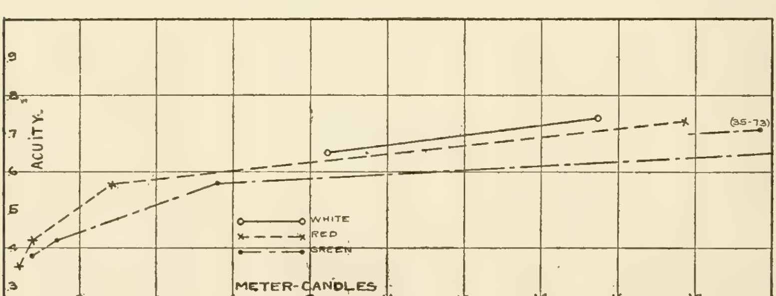


FIG 11

VI

DISCUSSION OF RESULTS

In order to emphasize the great diversity in the conclusions reached as a result of the various investigations already referred to, the following brief summary is given.

1. König and Broca and Laporte agree with Helmholtz in the view that there is no appreciable difference in acuity between illuminations of different colors, nor is there any difference between colored and colorless illumination. Failure to determine accurately the relative intensities of illumination constitutes the chief ground on which to question these conclusions.

2. Uhthoff and Örum support, in general, the conclusions of the present investigation, ascribing the highest efficiency to white illumination, with red, green and blue following in the order named.

3. Boltunow, on the other hand, while attributing the highest efficiency to white light, finds a decided advantage in acuity for green illumination, as compared with red. In criticism of his work it has already been pointed out that his test character was not well adapted to the purpose, and that the method of looking directly toward the source of illumination does not afford an accurate test of acuity. This method in reality involves the perception of brightness rather than the perception of form, a distinction which is considered elsewhere. This conclusion is in accord with the assertion of Engelmann[1] that in microscopic work green illumination yields better results than red, but in this kind of work also observations are made by means of transmitted light.

In seeking a theoretical explanation of the experimental data, one is impressed with the great diversity of the factors which enter into the problem. A few of these have already been made the subject of separate investigation, but it is clear that the relation of all to the main problem will have to be determined with much greater certainty before a satisfactory solution is forthcoming. The scope of the present work permits only an enumeration of the more important of these factors, together with, in some cases, a brief reference to the work already done in connection with them.

1. *The distinction between the form sense and the brightness sense* has already been referred to, and the failure to observe this

[1] Engelmann: cited by Boltunow, *Zeitschrift*, 1907–8, **42** (2), 359.

distinction has been suggested as a probable source of error in much of the work previously done in visual acuity. The partial independence of the form sense and the brightness sense is demonstrated by the familiar fact that the peripheral parts of the retina are unable to give impressions of form, whereas their sensitivity to light is, under certain conditions, even greater than that of the fovea.

According to the rod and cone theory of vision proposed by von Kries, the cones, which are found in greatest numbers in the fovea, are sensitive chiefly to color and form, as contrasted with luminosity, while the rods, which are predominantly the end-organs of the peripheral parts of the retina, yield only sensations of luminous intensity.[2]

In making this distinction we do not lose sight of the fact that for perception of detail we are largely dependent upon small differences of brightness, nor, on the other hand, of the fact that variations in luminosity may be perceived with strictly central fixation.

2. *The state of adaptation of the eye* has been clearly shown to affect acuity in general as well as the sensitivity of the eye to different colors. The sudden fall and subsequent increase in acuity in going from a brightly illuminated room into a comparatively dark one and Purkinje's phenomenon are familiar illustrations of this fact. It has been asserted[3] that for very low intensities green illumination yields a higher acuity than red.

3. *The sensitivity of the different parts of the retina to lights of different colors, in different states of adaptation.*—It has been shown that different parts of the retina do not show the same variations in sensitivity in changing from light to dark adaptation. Vaughan and Boltunow[4] find that in the state of light adaptation the sensitivity for red, green and blue light is by far the greatest at the fovea, and that it falls off rather quickly toward the periphery, about equally for all three colors. At 10 degrees from the fovea the sensitivity is about one fourth its central value, and at 20 degrees i· is from one tenth to one twentieth its central value.

In dark adaptation, on the contrary, the peripheral parts more sensitive to light than the fovea, and for green and blue t· stimulus value increases enormously for the peripheral parts. F· red, however, even after long adaptation, the sensitivity is less fo· the periphery than for the fovea. This fact is significant in connection with the explanation of the higher acuity for red illumination which will be proposed later.

[2] *Zeitschrift f. Psych. u. Physiol.*, 1896, **9**, 87.

[3] Dow, *Illuminating Engineer*, London, **2**, 233; Stuhr, *Ibid.*, p. 345.

[4] *Zeitschrift*, 1907–8, **42** (2), 1.

Pertz[5] has shown that at a distance of 1 degree from the fixation point the sensitivity of the retina in dark adaptation rises to almost 4 times its central value for blue light, while for red it falls to .70. At two and one half degrees the sensitivity for blue rises to 64 times the central value, and for red it falls to .55.

4. *Effect of accommodation, in near and distant vision.*—A number of observations on acuity have been made by J. S. Dow[6] which tend to show that with illumination of a given color the relative acuity is not the same for near and for distant vision. He concludes that for very near vision the blue end of the spectrum is the best, for moderate distances the central region, and for distant vision the red end. Dow's explanation involves the chromatic aberration of the eye, which will be considered in the next paragraph.

5. *The chromatic aberration of the eye.*—This defect of the eye as an optical instrument was first pointed out by Wollaston.[7] It may be strikingly demonstrated by means of a cobalt glass, or with the blue photographic filter, which transmits a narrow band of red in addition to the blue. Upon looking at a source of light of high luminosity, as an incandescent lamp, the filament is found to be bordered with red. If distant vision is used, the central part is red and the borders are blue. At intermediate distances, the red and blue colors overlap, and a single image of light purple hue is seen.

The following diagram, reproduced from Tscherning's "Physiologic Optics,"[8] illustrates the physical principle involved.

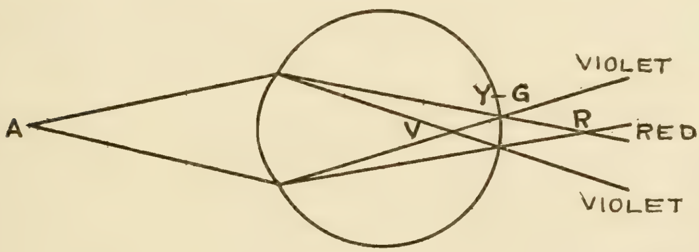


FIG 15.

ing the eye from *A* will undergo dispersion as they

[5] *Zeitschrift*, 1897, **15**, 327.

[6] *Electrical World*, 1911, **58**, 955.

[7] *Philosophical Transactions*, 1801, p. 50.

[8] Tscherning's "Physiologic Optics," Philadelphia, 1900.

pass through the refractive media, the violet rays being brought to a focus in front of the retina, and the red rays behind the retina. Under ordinary illumination the intensity of the extremes of the spectrum is so low, comparatively, that vision is not noticeably impaired. It is asserted by Dr. Louis Bell[9] and others, however, that with monochromatic sources of illumination the acuity is much higher than it is with white light. In the study referred to he found that illumination from a mercury vapor arc lamp showed a value, as judged by acuity measurements, roughly from one and one half to two times its real photometric value, when compared with light of continuous spectrum derived from ordinary incandescence. Reduced to Snellen's scale, this would amount to an increase of from twenty to twenty-five per cent.

Dr. Bell found a similar result with the flaming arc lamp, which is rich in red rays, and consequently he attributes the increased acuity solely to the monochromatic character of the illumination.[10]

6. In addition to the factors above mentioned and in close connection with them the question of *individual differences* must also be taken into account. As is pointed out in the section devoted to color photometry, it has been found that every degree of sensitivity to colors is shown, ranging from the so-called normal eye, to the absolutely color-blind. König[11] states that color-blind individuals have abnormally low acuity, which he attributes to the inefficiency of the cones.

Further, the dioptric structure of the eye will have an important effect upon the acuity with different colors. In view of the chromatic aberration of the eye, it is to be expected that the far-sighted person with average color-sensitivity would secure better acuity with green or blue illumination, while red would be more favorable to the near-sighted person.

7. *The size of the pupil* as affected by illuminations of different colors may be an additional factor entering into the problem of acuity. It is of course clear that with increased aperture both chromatic and spherical aberration will be increased, resulting in reduced acuity. The increased quantity of light admitted to the eye might, on the other hand, be sufficient to offset this effect. It has, in fact, been demonstrated by Hummelsheim[12] that between the limits of 1 and 50 meter-candles of intensity acuity, as determined by the use of the Snellen charts, with a narrow pupil considerably exceeds

[9] *Electrical World*, 1911, **57**, 1163.

[10] See also Luckiesch, *Electrical World*, Aug. 19, 1911.

[11] See page 11.

[12] *Arch. für Ophthalmol.*, 1898, **45** (2), 357.

acuity with a wider pupil. From 50 meter-candles upward the difference between the two is slight.

This enumeration serves to indicate the complexity of the problem. It suggests a very probable explanation of the wide variations in the conclusions reached by different observers on the basis of variations in essential details of procedure, and it emphasizes the importance of standardizing, or at least of clearly defining, the conditions of any experimental study, as to the form of test, the intensity of the illumination, the reading distance, the extent of the retinal field employed, the size of the pupil, the individual peculiarities of the observer, etc.

With respect to the present series of observations it may be said that all controllable conditions were kept as nearly as possible like those which usually obtain in the practical utilization of artificial illumination.

In support of the conclusion herein stated that light of the longer wave-lengths is more favorable to acuity than that of the shorter wave-lengths, attention is called to the following considerations.

As pointed out by König,[13] and in accordance with the theory of von Kries,[14] the rods alone are active with low intensities of illumination, whereas the cones come into play only with increased intensity. This fact must be taken in connection with the observations of Vaughan and Boltunow[15] and of Pertz[16] to the effect that under dark adaptation red shows a decrease in stimulative value in passing from the fovea to the peripheral parts of the retina, while green and blue show a decided increase. On the other hand, in daylight adaptation, in which cone vision predominates, the stimulative effect of all colors decreases at equal rates in passing from the fovea. Further, red illumination shows no photochromatic interval, and in dark adaptation weak red rays produce no sensation.

All these facts indicate that the cones are comparatively more sensitive to the longer wave-lengths, while the rods are comparatively more sensitive to the green-blue end of the spectrum.

Now inasmuch as the perception of form is peculiarly the function of the cones, it follows that under red illumination at comparatively low intensities the maximum stimulation of the cones is secured, while the stimulation of the rods remains at the minimum. In consequence we have an optimum perception of form, while the

[13] See page 11.
[14] *Zeitschrift*, 1894, **9**, 81.
[15] See page 36.
[16] See page 37.

sensation of brightness, which depends largely upon the stimulation of the rods, is at its minimum.

This difference in the stimulative value of red and green will be made effective in two different ways:

First, inasmuch as the brightness value of the illumination is dependent upon the sensation aroused in the rods, green illumination will have a relatively higher value than red, although this increased intensity will not be available for the cones in the perception of form.

Second, interference may be assumed to exist between the brightness sense and the form sense, so that an objectively constant stimulation of the form sense will be more or less effective, in proportion as the concurrent stimulation of the brightness sense is less or greater. Some support for this assumption of interference is found in the observed effect of a light which is brought into the peripheral part of the visual field when an object is being fixated with the fovea. Such a light always results in a reduction of acuity, even though its image may fall more than 50 degrees from the fovea.

According to the explanation here proposed, the efficiency of white light would depend upon the proportion of red or blue light which it might contain. With a high proportion or red, it might possibly equal or even surpass the efficiency of the monochromatic red. If, on the other hand, the green-blue components should predominate, its efficiency would fall below that of the red.

VII

COLOR PHOTOMETRY

Reference has already been made to the very obvious truth that in an adequate study of visual acuity the accurate determination of the intensity of illumination is a matter of the highest importance. So long as only a single color is involved, the variation of the intensity in fixed ratios in accordance with the law of inverse squares or by the use of the episkotister is a very simple matter. Or if different sources of light of the same hue are employed, their relative intensities may be determined with the more sensitive forms of photometers now in general use with an error of less than one per cent. It is possible, moreover, to make the measurements of intensity purely physical in character, if desired. The energy of the source may be determined directly by bolometric methods, or the chemical effects may be registered with minute shades of difference on the photographic plate. Quite recently the selenium cell has been proposed as affording more accurate and refined measurements of light intensity than any method hitherto used.

When, however, differences of color are involved, the problem of determining the comparative intensities of the lights becomes complicated with physiological and psychological difficulties of the most serious character.

In the studies of visual acuity already referred to, especially those of Uhthoff and König and of Broca and Laporte, these difficulties, which are generally recognized, were either entirely ignored or met in a wholly inadequate matter. König states that no attempt was made to reduce the intensities of the different colors to a common basis, and in Broca and Laporte's work the photometric measurements seem to have been made in the loosest possible way.

In the present study the complexities of color vision have been kept constantly in mind, and the determination of the comparative intensities of the lights of different colors has been regarded as the most important phase of the work. It is the purpose of this part of the report to state briefly the difficulties involved in color photometry and the various methods proposed for their solution, following this by an account of the method adopted in the present investigation, with arguments in support of the accuracy of the method.

Difficulties of a physiological character involved in estimating the comparative intensities of lights of different colors arise from the

varying sensitiveness of the eyes of different individuals to different wave-lengths, and from the varying sensitiveness of different parts of the same retina. These variations among individuals range from cases of pronounced color-blindness of well-recognized types to minor differences which are discovered only by careful comparisons. Because of these variations it is practically impossible for one observer to check up the results obtained by another, unless they happen to be so situated that the observations of both may be referred to some recognized common standard.

The existence of these individual differences in color vision was first pointed out by Professor Rood[1] in a study of his own color sensitivity as compared with that of eleven other persons. Professor Rood asserts that prior to this study "in the matter of color vision there seems to have been a tacit assumption that all persons could be divided into two classes, those with normal vision, and the color-blind. Holmgren's test with colored worsteds classifies them in this way, and analogous tests give a like result. According to this view the color vision of persons free from color-blindness has generally been considered to be alike."

In order to test this theory Professor Rood compared his own color vision with that of others, and found that while none agreed with him, they also diverged from one another. These divergences proved to be too large to permit of being attributed to errors of observation. On the basis of the results obtained, Rood found that his subjects fell naturally into two groups, equally large, with respect to their perception of green. The one group comprised those who had the highest sensitivity in the green, and the other group those who had the least sensitivity in the same color, and the highest in either the red or the blue. Inasmuch as there was no reason apparent why either group should be considered as normal rather than the other, the mean color vision of the eleven observers was taken as representing the normal, and the divergence of each person from the standard was then calculated.

At the same time tests were made of the vision of three persons who were color-blind to red; in two of the cases the defect was not previously suspected.

The following tables give the sensitivity to different colors of the various observers, the maximum sensitivity attainable in each case being represented by the number 100.

[1] *Am. Jour. Sci.*, 1899, No. 158, p. 258.

Class A—Minimum Sensitivity in the Green

	Red	Green	Viol.-blue
Trowbridge	100.0	91.6	95.6
Wade	97.7	97.4	100.0
Hallock	100.0	90.9	96.2
Furness	97.9	90.8	100.0
Curtis	90.5	86.6	100.0
Miss M.	100.0	81.6	99.0

Class B—Maximum Sensitivity in the Green

	Red	Green	Viol. blue
White	96.1	100	95.8
Parker	85.7	100	95.8
Dennett	93.8	100	91.5
Tufts	89.9	100	87.8
Day	82.9	100	93.3

Color-blind Group

	Red	Green	Blue
Alsberg	30.3	88.1	100.0
Mr. W.	35.6	85.5	100.0
Mr. O.	35.3	100.0	93.9

These results clearly indicate that wide variations in the determinations of the luminosity of different colors by different individuals are to be expected from purely physiological causes, and that in consequence it is necessary to know the peculiarities of color vision of the individual observer before his results can be of any value for purposes of comparison. These errors, however, for a given individual may be regarded as practically constant, and corrections may be made for them by reference to a fixed standard, obtained, as Professor Rood suggested, by adopting the mean of a great number of observations.

Difficulties which have a psychological basis, on the other hand, give rise to discrepancies which are much more serious, and which at the same time seem incapable of elimination. They arise from the well-known fact that color sensations are not simple, but on the contrary highly complex, and that it is practically impossible in ordinary experience to analyze them into their component elements. There are involved, in the first place, all the variations in hue which are found as we pass from the red end of the spectrum to the violet. It is a matter of common observation that the colors in the lower part of the spectrum, the reds and oranges, produce an impression of greater warmth or ''liveliness'' than the hues of shorter wave-length, and this impression the observer is naturally disposed to interpret in terms of brightness, which constitutes a second and quite distinct quality of color sensations.

A third element of color sensation is that commonly designated as saturation, depending upon the proportion of the black-white sensation which enters into the whole complex. From a physical point of view these different qualities of color sensation are dependent, respectively, upon the absolute wave-length, the amplitude of the

vibration or the energy of the light wave, and the complexity of the wave-form. Now we know that change of wave-length brings with it variation in both brightness and saturation, and change of energy or brightness is accompanied by changes in both hue and saturation. It is therefore practically impossible even for a trained observer to eliminate the effects of hue and saturation in making judgments of brightness, the quality with which we are concerned in color photometry.

Notwithstanding this inherent difficulty, a very large proportion of investigators who have attempted to make comparisons of the intensities of lights of different colors have adopted some form of the direct method, in which the observer relies solely upon his unaided judgment. In the so-called indirect methods the effort is made either to eliminate entirely the disturbing factor of the color-tone, or at least to control it more carefully. Of the direct methods Helmholtz makes the following criticism: "I must explain that personally I put no confidence in my judgment concerning the equality in luminosity of differently colored surfaces. I admit, however, that of two differently colored fields, one can be so much darkened that there remains no doubt that the other is brighter."[2]

Helmholtz views with considerable scepticism the constant values obtained by certain observers, and says of them: "Observers who have trained themselves in the same observations attain finally a somewhat greater certainty, but this might indeed be secured as well through constant practise or greater attention to various accessory influences."[3]

Von Kries has practically the same criticism to offer concerning the direct methods, but is even less favorable to the indirect methods. Of the latter he says: "Whatever else may be said of these methods, it is at least certain that for each pair of lights found to be equally bright, some definite physiological relation exists; what this relation is, however, is not at all certain. Above all, it does not seem appropriate that one should speak of these as 'methods for the comparison of the brightness of different colored lights.' Even if the physiological relation which exists were known, yet the last argument that the method really measures the brightness must be found in the fact that the values obtained coincide with those obtained by direct comparison. The direct comparison has the last word."[4]

Space will not permit reference to the many attempts that have been made to compare the intensities of lights of different colors by

[2] "Handbuch der physiologischen Optik," 2d ed., p. 440.

[3] "Handbuch der physiologischen Optik," 2d ed., p. 428.

[4] Nagel's "Handbuch der Physiol. des Menschen," Vol. 3, p. 259.

the direct method. The general method of procedure and character of results are well exemplified in the most recent and at the same time most exhaustive study of this sort which was conducted by Dr. Herbert Sidney Langfeld in 1908 in the Psychological Institute of the University of Berlin, under the direction of Professor Carl Stumpf.[5]

Accepting the view of Von Kries as to the superiority of the direct method over the indirect, Dr. Langfeld chose the former, comparing each color successively with different shades of gray. He explains that he does not regard brightness merely as the intensity of any radiation which may be measured by physical means, but solely as a quality which attaches to color as a phenomenon. By brightness he understands a simple experience, which can not be described in other terms, and which is to be illustrated only by reference to results obtained by experiment. It must be ascribed to "tonfreie" as well as to "bunte" "Farben." In "tone-free colors," that is, the grays, the nuance varies along with the brightness from absolute white to absolute black. The variations in nuance are qualitative, those in brightness are quantitative. Therefore, whenever we determine the brightness of a color, whether it is a tone-free, a saturated or an unsaturated color, the brightness is in all cases the same property. It is not correct to say, as is often done, that the brightness is the gray contained in the color.

In his apparatus Dr. Langfeld employed the principle of Hering's "Nuancierungsapparat." The observer looked through an aperture in a gray cardboard upon a field in which could be made to appear either the colored paper whose brightness was to be determined, or a screen of gray paper whose brightness could be varied by rotating it on a horizontal axis. The fields were illuminated by a Nernst lamp of 150 candle power, whose rays were passed through a blue glass in order to weaken the yellow components.

In discussing his results, Dr. Langfeld says that he found that his judgments of brightness, as was true also in the cases of the other observers, might be of two distinct kinds. He says: "Either I could compare the colors, by attending to the light which seemed to come from them, in which process I abstracted from the color-tone, which remained in the background of consciousness, or I could devote my attention to the colors as such. . . . In each case I had an impression of brightness, but each impression was quite distinct." He quotes numerous expressions from his observers which indicate that they experienced the same difficulties throughout the series of observa-

[5] *Zeitschrift f. Psychol.*, 1909, **53**, 113–178.

tions. Some of them recognized the distinction between the two classes of judgments. Others only felt the disconcerting effects without understanding their cause.

It is on the basis of this distinction that Langfeld explains the great scattering of his values, as well as of the values obtained by the others. With certain colors the impression of the color-tone would predominate, with others the impression of the brightness as such. Thus in the blue and the yellow the values are much scattered, but they concentrate about two points, the blue at 32 and 52 (the numbers indicate the degrees of rotation of the gray cardboard to match the given color in brightness), and the yellow at 57 and 80. At one time the one attitude predominated, at another time the other. In the violet only one standard of judgment was used, and in other colors both impressions combined in such a way that the values were spread out over a wide range, with no concentration at particular points.

The accompanying excerpts from the tables of results obtained will indicate the wide variations in the values. The upper line of figures for each observer gives the limits of the angles in degrees, and the second line gives the actual brightness values, in terms of the cosines of the angles.

Obs.	Red	Orange	Yellow	Green	Green-blue	Blue	Violet
L.	40–76	39–80	54–85	44–81	30–53	30–54	27–35
	(76–24)	(76–17)	(58–9)	(72–15)	(87–60)	(87–58)	(89–82)
G.	40–86	54–75	56–74	57–80	46–84	31–62	27–45
	(76–7)	(58–25)	(57–27)	(54–18)	(69–10)	(86–47)	(89–70)

Dr. Langfeld states that the most important fact to be learned from the tables is that in general two very different sets of figures are obtained from the two different standards of judgment. "Under ordinary conditions such as obtained in the researches conducted by other investigators, consistency and certainty could not be secured, except when the observers by chance adopted the same 'attitude.' But it can not be assumed from this fact that heterochromatic brightness comparison is impossible. We must maintain, on the strength of our experiments, that such comparison is possible, and that satisfactory results may be obtained if observers adopt a definite 'attitude,' and train themselves to hold to this consistently."

In a subsequent attempt to compare the brightness of spectral colors with Helmholtz's color mixing apparatus, Langfeld found that when he made the comparison on the basis of the color-tone, red was brighter than orange, and both were brighter than yellow. When the other basis of judgment was used, these three colors followed the natural order of brightness, red, orange, yellow.

However hopeful Dr. Langfeld may be as to the possibility of making these comparisons by direct methods, his results seem to be of value only in demonstrating the uncertainties that are necessarily associated with this method, and in explaining the cause of these uncertainties. Physicists meet with the same sort of difficulties in attempting to make comparisons of differently colored lights with photometers of the direct comparison type.

It was with the purpose of avoiding, if possible, these subjective difficulties that the principle of the flicker photometer was adopted in the present investigation, in preference to the direct comparison photometers in common use. This principle, however, has not as yet received much favor at the hands of physicists, and it is necessary therefore to enter at some length into the discussion of the principles involved and the results obtained which would seem to justify its use.[6]

The flicker method of photometry was first suggested by Professor Rood in a paper published in the *American Journal of Science* in September, 1893. Apparatus for the application of the principle was constantly improved and tested by Professor Rood until 1899, when he stated as the conclusion of his investigations that ''the accuracy attainable with the flicker photometer, as at present constructed and using lights of different colors almost spectral in hue,

[6] Since this chapter on photometry was written there has come to be more general agreement among physicists as to the trustworthiness of the flicker method in heterochromatic photometry. The following quotations represent the view held by many investigators at the present time.

Dr. Louis Bell says: ''I am strongly of the opinion that one makes no mistake in assuming the substantial correctness of the flicker method where heterochromatic comparisons are to be made—that is, if in a heterochromatic comparison there is a substantial difference between the results obtained with the flicker instrument and with an equality-of-brightness photometer, it is altogether probable that it is the latter which is in error.'' (*Electrical World*, 1911, **58**, 637.)

Mr. Herbert E. Ives, of the laboratories of the National Electric Lamp Association, who recently made a careful study of the flicker and equality-of-brightness methods of photometry, found that ''the flicker method is for all parts of the spectrum several times as sensitive as the equality-of-brightness method,'' and that inexperienced observers with the flicker photometer are able to make determinations which agree very closely with those of experienced observers, whereas with the direct comparison methods their variations may be from five to ten times as great. He states as his conclusion that ''the much greater sensibility and ease of setting of the flicker method point to its decided superiority for heterochromatic photometry.'' (*Transactions of the Illuminating Engineering Society*, 1910, **5**, 711.)

is about the same as with ordinary photometers using plain white light, or light of exactly the same color.'"[7]

Following the work of Rood the principle of the flicker was further tested out by Professor Frank P. Whitman,[8] using colored papers of the Milton Bradley series and colored glasses. The precision of setting possible with the apparatus was first tested by making over 100 observations with nineteen different colors, covering the whole range of the spectrum. The difference between two successive readings was seldom more than one per cent., although in a few cases it ran as much as two or three per cent.

The second test was concerned with the comparative values obtained by different observers. Two lamps were balanced against each other by ordinary photometric methods at respective distances from the photometer head of 2.98 feet and 3.02 feet. The same values were obtained by each observer. By the flicker method the distances were 2.96 and 3.04 feet for one observer, and 2.98 and 3.02 for the other. A red glass was then interposed on one side and a green glass on the other. The readings than became as follows:

	Direct	Flicker
Observer 1	2.59–3.41	3.79–2.21
Observer 2	3.08–2.92	3.88–2.12

These results show, first, that when lights of exactly the same color are compared and brightness only is involved, two observers who get indentical values by direct methods will get practically identical results by the flicker method; and, second, that where color differences are involved, the values of different observers by the direct method may vary widely, in this case by almost 20 per cent., whereas by the flicker method the difference is very slight, in this case only a little over 2 per cent.

A third series of experiments made by Whitman demonstrated the fact that the values obtained with bright and with faint illuminations did not appreciably differ, and that thus the difficulties associated with Purkinje's phenomenon and met with in direct comparisons are avoided with the flicker method.

The most important point established by Whitman was that the flicker method gives a true measure of luminosity comparable with that obtained in other trustworthy ways. The luminosity values of differently colored papers were determined by means of the flicker photometer and by the use of the Maxwell discs, with the following results:

[7] *Amer. Jour. Sci.*, 1899, No. 158, p. 194.

[8] *Physical Review*, 1896, **3**, 241.

	Red	Green	Blue	White
Color equation—Maxwell discs ...	40.5	49.2	10.3	22.6
Luminosity, by flicker	.238	.295	.106	
Luminosity value	9.64	14.50	1.09	
Total luminosity of colors				25.23

	Red	Green-yellow	Blue	White
Color equation—Maxwell discs ...	18.5	34.0	47.5	30.4
Luminosity, by flicker	.238	.617	.106	
Luminosity value	4.41	20.96	5.03	
Total luminosity of colors				30.4

The upper line of numbers in each table represents the percentage of the whole disc occupied by the given color, the combination of the three colors being matched against the white-black of the central part. "The amount of white light in the black-white combination, corrected for the light reflected by the black portion, is the measure of the luminosity of the colored discs in terms of white, which quantity, again, is dependent upon the luminosities of the three colors of which it is composed.

"If now the fraction of the whole circle occupied by any color is multiplied by its luminosity as measured with the flicker photometer, the result will be the amount of white equivalent to that colored sector, and the sum of the results obtained by treating each of the colored sectors in this way should equal the amount of white in the black-white disc." "Fourteen such trials were made with different colors, the results differing by one to three per cent. from exact equality."

More recently Professor Tufts,[9] of Columbia University, applied the principle of the flicker photometer to the determination of the relative luminosities of the different parts of the spectrum, and demonstrated that the sum of the luminosities of the spectrum colors determined in this way is equal to the measured luminosity of the original white to within about 3 per cent., which is well within the error of spectrophotometric measurements.

In the course of the same experiments Professor Tufts demonstrated the fact that the luminosity sense of the eye is practically independent of the color sense. He first determined his own wave-length luminosity curves for both eyes, and found them to be practically identical. He next exposed his right eye to light of a given color until it was fatigued to such an extent that all white objects viewed with that eye appeared of a complementary color. He then measured the luminosity of the given spectrum color with the fatigued

[9] *Physical Review*, 1907, **25**, No. 6.

eye, and immediately after with the unfatigued eye. This was done for seven different positions in the spectrum. With one exception, that of prolonged exposure to red, the fatiguing of the eye by any color did not produce any change in the luminosity sense. Two other observers with normal color vision made the same observations and obtained similar results.

This observed independence of the luminosity sense and the color sense harmonizes well with von Kries' theory[10] that the cones are a color-perceiving apparatus, while the rods are insensitive to color, and give only sensations of luminosity. This distinction is also strikingly brought out by certain interesting experiments of Lummer.[11] By throwing a spectrum on a white screen, starting with low illumination and gradually increasing its intensity, he has shown that the first recognizable light is colorless, with the apparent maximum intensity in the blue-green. As the illumination is increased, sensations of color arise, and the point of maximum luminosity gradually shifts toward the yellow-green region, showing that the rods are most sensitive to the blue-green end of the spectrum, although they are unable to produce sensations of color as such. Lummer also shows that if a platinum filament is heated to a dull reddish luminosity and is observed directly, it appears red and sharply defined, but if observed obliquely, it appears brighter, loses its color and at the same time loses sharpness of contour.

The greater constancy of the flicker method of photometry as compared with direct comparison methods is clearly brought out in a comparative test recently made by L. W. Wild,[12] an English physicist, who has devoted considerable attention to the problem of heterochromatic photometry. He introduces his discussion with the general statement, confirmed by his experience, that "the great merit of the flicker type is that the same reading within about one per cent. can be obtained again and again, even though the lights differ in color to a considerable extent, whereas with photometers of the equality of brightness type under the same conditions of color differences the readings may lie 10 per cent. or more on either side of the mean, and consequently a very large number of readings must be taken in order to secure a true average."

In his tests Wild used a carbon filament lamp and one with a tungsten filament. Six different photometer heads were employed three based on the flicker principle, and three on the equality of brightness principle. The following table gives the maximum, the

[10] *Zeitschrift f. Psych. u. Phys., d. Sin.*, 1894, **9**, 81–123.

[11] *Report of Smithsonian Institution*, 1904, **1**, 249.

[12] *The Electrician*, 1909, No. 63, p. 540.

minimum and the mean scale readings. Ten readings were taken with each flicker head, 20 with the Bunsen, and 50 with the Lummer-Brodhun.

	Flickers			Contrast	Equality	
	Wild	Whitman	Simmance	Bunsen	Prism	Lummer
Maximum	1.73	1.735	1.735	1.86	2.05	2.00
Minimum	1.72	1.715	1.71	1.79	1.72	1.70
Mean	1.725	1.722	1.722	1.825	1.85	1.83

As will be seen from the table, all three photometers of the flicker type give exactly the same mean values, and the range between the maximum and the minimum for each instrument is very small. On the other hand the three direct comparison photometers not only show wide variations between the maximum and the minimum readings in each case, but their agreement with one another in the mean readings is not nearly so close.

The conclusion drawn by Mr. Wild from the results of this study illustrates the common tendency among physicists to favor the direct comparison type of photometer as against the flicker type, even though the latter invariably yields the more consistent results when comparative tests are made. In this case the flicker instruments give the tested tungsten lamp a value about 6 per cent. less than that given by the other type, and for this reason the author concludes that "in spite of the greater sensitiveness of the flicker photometer, it will have to be discarded in favor of the Bunsen disc."

In order to test more fully the reliability of the flicker method in color photometry, the writer made an exhaustive series of observations with colored papers of the Hering series, using the photometer head as originally designed by Professor Rood and improved by Professor Tufts. The apparatus was further modified by the writer to meet the requirements of this particular test.

The principle on which the use of the flicker photometer is based may be briefly stated as follows: When the same point on the retina is alternately stimulated by light coming from two unequally illuminated and differently colored surfaces, a sensation of flicker results, which is due to differences in both intensity of illumination and color. By increasing the speed of alternation a point is reached at which the flicker resulting from the color difference disappears because of optical fusion. The intensity flicker, however, persists to a much higher frequency of alternation, unless the two illuminations are equalized. The speed of alternation at which the color flicker disappears varies with the intensity of illumination, but under any conditions the color flicker disappears before the intensity flicker.

Assuming the correctness of this formulation of the principle of flicker, a satisfactory means is thus afforded of eliminating the influence of color in making comparisons of intensity, an influence which, as has already been pointed out, is always noticeably present when such comparisons are attempted by direct methods.

Description of Apparatus

The essential parts of the photometer (Fig. 16) are contained in a cubical box, approximately 8 inches on a side. This box is mounted on an ordinary photometrical bench, which is conveniently graduated. Light from the two sources under comparison enters the box on either side through apertures 2 inches in diameter, and falls upon two vertical planes which are inclined to each other at a fixed angle of 90 degrees. The two planes are permanently fastened together in the form of a wedge, with the edge directed to the front of the photometer.

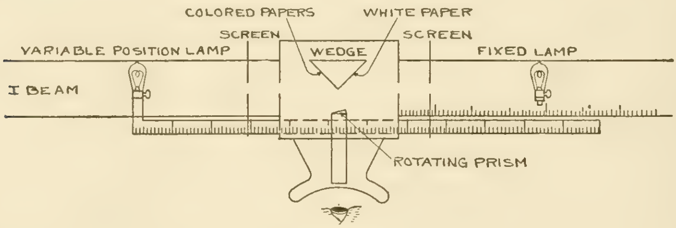


FIG 16.

Both faces of the wedge contain slots for holding small sheets of paper, either pure white or colored. By allowing the edge of the paper on one side to project slightly beyond the exact edge of the wedge, the margin of the paper on the other side is concealed, thus forming a very keen line of division between the two planes.

When the photometer head is to be used in the ordinary way, that is, to measure the relative intensity of two lights of the same color, sheets of white paper cut from the same piece are inserted in either side of the wedge. With this arrangement also lights of different colors may be compared.

In this series of observations, however, differences in color were secured not by varying the sources of light themselves, but by substituting papers of different colors for the white sheets on the faces of the wedge.

The wedge is viewed from the front through a brass tube about 9 inches long and .75 inch in diameter. The axis of this tube lies along the line which bisects the angle formed by the two planes of the wedge. At the outer end of the tube and at certain points along its length are inserted diaphragms by means of which the diameter is stopped down to about .25 inch, thus preventing reflection from the inner surface of the tube.

The inner end of this tube is about 3 inches away from the edge of the wedge, and carries a small 10-degree prism by means of which the rays of light coming from the wedge are refracted on their way to the eye. The portion of the tube which contains the prism is capable of rotation, and in consequence of this rotation the two planes of the wedge occupy the field of vision alternately in varying proportions from totality to zero. The line of division between the two planes is seen to move across the field from right to left and vice versa.

Figure 17 indicates the path of the rays of light from the wedge to the eye at different phases of the rotation of the prism, and the corresponding appearance of the field.

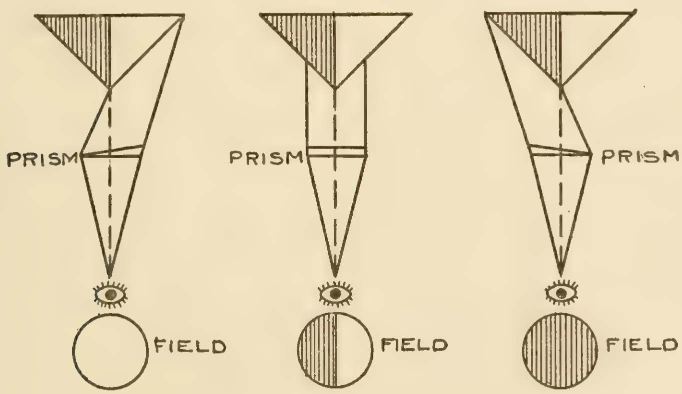


FIG. 17.

The rotation of the prism is accomplished by means of an electric motor, the speed of which may be varied within desired limits.

The eye of the observer is protected from extraneous light by a hood which is capable of being moved from side to side so that either eye may be used in the observations.

The sources of light used in this series of experiments were two carbon-filament incandescent lamps of nominally 16 candle power,

placed on the same circuit, their voltages being capable of independent variation. No effort was made to avoid momentary fluctuations in the voltage of the circuit during the observations, as both lamps were equally affected by them, and fixed values of illumination were not required.

Photometric Observations

The method of procedure was as follows: white papers were first placed upon both faces of the wedge, and the lamp to the right, which was to remain fixed, was placed at a determined distance from the photometer head, say 150 cm. The variable-position lamp was then placed at the same distance and the prism was set in rotation, the proper speed being determined by observation. If a flicker appeared with this setting, the variable-position lamp was moved further away or nearer until the flicker disappeared entirely, or was reduced to a minimum. A graduated rod running from the movable lamp under and past the center of the wedge carried a strip of white paper. In this paper a punch mark was made when the proper setting was obtained, and this procedure was repeated nine times. The mean of the ten readings was taken as the correct distance of the variable-position lamp to balance the fixed lamp.

By putting a standard lamp on the variable-position side and comparing its balancing distance with that of the first lamp used, the candle power of the latter could readily be determined. In this series the desire was not to determine the candle power of the lamp, but to so adjust the intensities of the two lamps that a photometric balance would be secured with the lamps at equal distances from the photometer head. This adjustment was secured by varying the voltage of one of the lamps. Comparisons were made also at 100 cm. and at 75 cm., until it was absolutely certain that the two lamps gave equal intensities of illumination at the same distance.

Having secured in this way a satisfactory balance of the lamps, a sheet of red paper was substituted for the white on the left side of the wedge, the fixed lamp was again set at 150 cm., and the variable-position lamp was moved backward and forward until the flicker was eliminated. As before, the mean of ten observations was taken. This procedure was repeated for distances of 100 cm. and 75 cm. In this way ten colors chosen from the Hering standard series were compared with white and their brightness values were determined in terms of the latter.

The following table (XVIII.) shows the distances at which the variable-position lamp was set with the different colors in order to balance the white when illuminated with the fixed lamp at distances of 150 cm., 100 cm., and 75 cm.

TABLE XVIII

Colors	*Fixed Lamp at 150 Cm.* Bal'g Dist.	A. D. in Per Cent.	*Fixed Lamp at 100 Cm.* Bal'g Dist.	A. D. in Per Cent.	*Fixed Lamp at 75 Cm.* Bal'g Dist.	A. D. in Per Cent.
White	150.0		100.0		75.0	
Hering red	88.6	.56	58.5	.43	43.7	.68
Brick red	79.8	.46	53.3	.47	39.6	.80
Orange	114.2	.44	75.8	.65	56.7	.66
Orange-yellow ..	139.5	.54	93.7		69.8	.36
Yellow	145.3	.51	96.7	.38	72.5	.34
Green	88.5	.56	58.7	.88	44.0	.57
Blue-green	70.5	.71	46.5	.81	35.0	.72
Blue	60.2	.41	40.0	.31	29.9	.42
Indigo	52.4	.96	34.8	1.08	25.8	1.45
Violet	53.0	.47	35.1	.72	26.1	.48

Inasmuch as the consistency of the observations with one another affords the most available means of judging of the accuracy of the observations, it is of interest to note that in only two cases do the average deviations exceed one per cent., and that the average of all the average deviations is only a little over one half of one per cent.

The following table (XIX.) shows the comparative brightness values of the different colors as determined at different distances, on the basis of a value of 100 assigned to the white. The values of the colors are, of course, proportional to the squares of the distances of the lamps which illuminate them.

TABLE XIX

	Fixed Lamp at 150 Cm.	100 Cm.	75 Cm.	Average	A. D. in Per Cent.
White	100.0	100.0	100.0	100.0	
Hering red	34.8	34.2	33.9	34.3	.9
Brick red	28.3	28.4	27.9	28.2	.7
Orange	57.9	57.4	57.2	57.5	.4
Orange-yellow ...	86.4	87.8	86.6	86.9	.7
Yellow	93.8	93.5	93.4	93.6	.2
Green	34.8	34.4	34.4	34.5	.5
Blue-green	22.1	21.6	21.8	21.8	.8
Blue	16.1	16.0	15.9	16.0	.4
Indigo	12.0	12.1	11.8	12.0	.8
Violet	12.5	12.3	12.1	12.3	1.1

These results indicate that wide variations in the intensity of illumination do not produce noticeable changes in the comparative brightness values of the colored papers. The illumination at 75 cm. was four times as great as that at 150 cm., but notwithstanding this fact the deviations from the average values are less than one per cent. This is in striking contrast with results obtained from direct comparisons, in which, as has already been pointed out, the deviations run as high as 10 per cent. (p. 50).

In addition to comparing each of the colors separately with white, the conditions were made still more rigid by balancing the various colors against each other directly. For example, the red paper was placed on the right side of the wedge, and its lamp was set at the distance required by the red to balance the white at 100 cm. —viz., 88.6 cm. The other colors and the red itself were then inserted successively on the left face of the wedge. If the method were strictly accurate, each color should have its lamp at the same distance as when that color was balanced against the white at 100 cm.

Table XX. gives the distances for the various colors on the right side when balanced against red, green and violet placed successively on the left side.

TABLE XX

	Against Red at 88.6 Cm.		*Against Green at 88.5 Cm.*		*Against Violet at 53 Cm.*	
	Dist.	A. D. in Per Cent.	Dist.	A. D. in Per Cent.	Dist.	A. D. in Per Cent.
Hering red ..	89.0	.14			89.0	.42
Orange	115.5	.44	115.0	.32	115.2	.54
Yellow	146.3	.41	145.3	.34	145.2	.34
Green	89.1	.70	89.1	.70	88.2	.56
Blue-green ...					70.9	.70
Blue	61.4	.61	61.5	.81	60.8	1.23
Indigo	52.9				52.9	.48
Violet					52.4	.48

It will be observed that green when balanced against red shows no higher percentage of average deviation in the readings than when it is balanced against itself. In each case the average deviation, which measures the variability of the series of observations, is .7 per cent. This means that with this form of photometer differences in color of the fields compared, even when the difference is as great as that which exists between complementary colors, as red and green, produce no appreciable influence in the results.

Table XXI. gives the brightness values of the colors in this series of observations reduced to the same basis as in Table XIX. The values as determined in Table XIX. are reproduced for purposes of comparison.

TABLE XXI

	Balanced Against			
	Red	Green	Violet	White (Table XIX.)
Hering red	35.2		35.2	34.3
Orange	59.3	58.8	59.0	57.5
Yellow	95.1	93.8	93.8	93.6
Green	35.3	35.3	35.7	34.5
Blue-green			22.3	21.8
Blue	16.7	16.8	16.4	16.0
Indigo	12.4		12.4	12.0
Violet			12.2	12.3

The fact that practically all the values as determined in this series run slightly higher than those obtained in the original series would raise the suspicion that in spite of the precautions taken the photometer was not absolutely in balance. It was found, for example, in subsequent tests of the apparatus that it was so sensitive that a variation of more than one per cent. might be caused by having the grain of the paper used on the face of the wedge run in the wrong direction.

The belief in the accuracy of the flicker method in determining the brightness value of lights of different colors receives further confirmation in the work of Polimanti.[1] He studied the distribution of intensity in the spectrum by means of the flicker method and also by peripheral vision, and found that the distribution of the flicker values coincides approximately with the peripherally determined values, and that both are one and the same function of the wavelength.

[13] *Zeitschrift f. Physiologie der Sinnesorgane,* 1889, **19**, 263.

VIII

CONCLUSION

In summarizing the results of this study, attention is directed to the following points:

1. As regards the relation of acuity to intensity, it is shown that with uncolored illumination approximately 75 per cent. of daylight acuity is attained with an intensity of from 8 to 10 meter-candles; with a reduction of intensity below this point the acuity decreases rapidly, and with an increase of intensity beyond this point the acuity rises very slowly, unit acuity being attained with an intensity of from 40 to 50 meter-candles

As in daylight vision, after unit acuity is attained, further increase of intensity shows practically no gain in acuity. It may therefore be considered that intensities of 8 and 40 meter-candles constitute approximately the lower and upper limits, respectively, of suitable illumination for ordinary purposes.

2. As to the relative efficiency of red and green or blue illumination, the advantage lies decidedly with the red. Monochromatic yellow does not enter into this comparison. So-called white illumination gives a slightly higher acuity than red, but inasmuch as white illumination is predominantly yellow, it is quite possible that monochromatic yellow would have the same influence on acuity as white.

3. A probable explanation of the greater efficiency of red illumination is to be found in the apparently greater sensitivity of the form perceiving end-organs, the cones, to light of longer wave-length, as compared with the brightness perceiving elements, the rods, which have been shown to be more sensitive to the shorter wave-lengths.

4. In the study of visual acuity many distinct factors are involved, failure to regard any of which will seriously affect the results obtained. Among these the most important are:

(*a*) *Photometric determinations.*—Accurate estimation of relative intensities of colored illumination by direct methods is exceedingly difficult, if not impossible, and the flicker method is proposed as the only satisfactory one for making heterochromatic comparisons;

(*b*) *Test-characters.*—Care should be taken that the test-character employed actually measures the form sense rather than the brightness sense, and that it involves the use of the eye under condi-

tions approximately similar to those under which the latter is commonly used;

(*c*) *Individual differences.*—The demonstrated existence of great individual variations makes it imperative that the visual peculiarities of any observer be definitely determined. Value can attach only to results which represent the average of a number of observers.

THE CURVE OF FORGETTING

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CONTENTS.

THE CURVE OF FORGETTING

CHAPTER I

Historical Survey

The problems that present themselves in connection with the topic of forgetting are (1) the rate at which forgetting progresses, (2) the form of the curve showing the relation of forgetting to the lapse of time, and (3) whether there are any constant relations between either the rate of forgetting or the shape of this curve, on the one hand, and, on the other, the kinds of subject matter or the kinds of reagents, or the amount or manner of the practise or learning.

The first application of modern scientific methods in the study of memory and of forgetting was the work of Ebbinghaus, which he published in 1885.[1] Ebbinghaus assumed that when experience has weakened in its susceptibility to recall the remaining association strength can be measured best by the number of repetitions that it saves in relearning. Another person served him as subject in preliminary experiments; but in the principal experiments Ebbinghaus acted both as experimenter and subject. As he admits, his results are, therefore, of much less value especially for general psychology. The material learned in the experiments in forgetting consisted of 163 series, each containing 13 nonsense syllables. Every syllable contained a consonant, a vowel and then another consonant. Ebbinghaus took especial pains to avoid any succession of letters or of syllables that might suggest devices in learning. He read a series and then recalled as many syllables as possible until he reproduced the whole series once without an error. Whenever a series could not be promptly recalled in its entirety he read the remainder from the paper. The rate was kept constant at 150 syllables per minute by means of a metronome. Seven of the series were memorized one day and one was relearned at each of seven intervals of various lengths. The data of the shorter periods were the average results of from 12 to 16 series, and those of the larger intervals of from 22 to 26 series.

The results of this investigation of Ebbinghaus I present in the form of a table of arrays and a table of central tendencies and meas-

[1] "Uber das Gedächtniss," Leipzig, 1885.

TABLE I

RESULTS OF EBBINGHAUS

The Arrays of the Per Cents. Retained

Per Cent.	20 Minutes	1 Hour	9 Hours	1 Day	2 Days	9 Days	31 Days
60	5						
55	5						
50	1	4					
45	0	5	1	3	1		
40	1	3	1	2	3	1	1
35		3	6	9	1	3	2
30		1	2	4	7	6	7
25			2	3	4	3	6
20				2	3	7	8
15				2	4	3	9
10				1	2	0	6
5					1	2	6
0–5						1	
Average Deviations	4.3	5.1	4.3	7.2	9.0	10.4	7.9
Probable Errors ..	2.1	4.2	0.8	3.7	7.6	7.9	7.0
Averages	58.2	44.2	35.2	33.7	27.8	25.4	21.1
Modes	55–60	45	35	35–	30–	20–30	15

ures of variability. It can be seen that the portions of the work that were necessary to be performed again were approximately, after 20 minutes 1/3, after 1 hour 1/2, after 9 hours 2/3—, after 24 hours 2/3 +, after 6 days 3/4, and after 31 days 4/5. His statement of his conclusions from these results is, "The ratio of what is retained to what is forgotten varies inversely as the logarithm of the time."

Besides being the first experimental research in this group of phenomena, this was the first very extended laboratory investigation of the mental processes that are not closely paralleled by definite, commensurate, physical stimuli. In addition to this, all who have carried on experimental investigations in memory, and most especially in forgetting, will agree that the task as it was performed by Ebbinghaus required closeness of application and patient persistence that was nothing less than heroic. However, the fact that his work has more than one claim to a place among pioneer achievements is sufficient excuse for several imperfections in his methods.

There is good reason for believing that his work might have been more valuable for general psychology had he, with equal diligence, experimented upon several other persons. This most usual criticism of his method is atoned for in a measure by the fact that one could scarcely have been as severely exacting in his methodological requirements in that case as Ebbinghaus was with himself. Besides, learning depends so largely upon attention that any one to whom the problem was not of primary interest could not be so constant in

conditions for reaction as the one to whom the research was all-important. The aim of Ebbinghaus was merely to break the ground, thus to disclose only the predominant, and therefore the most common, factors in learning and in forgetting. Of course, there was a greater possibility of mistaking some individual factors for general phenomena than there would have been in case of a number of subjects. There is indeed no assurance that the regularity in this curve was in no degree due to his knowledge of the problem and of the results to be obtained. Therefore, although there were some advantages in experimenting upon himself, there is ample reason for believing that there is considerable justness in the criticism. The subject matter that Ebbinghaus supposed to be as simple as any that could be found was probably not so simple in possibilities for association or so single in forms of imagery as material that has been chosen by several other investigators more recently.

In reading the syllables from a paper he did not avoid the possibility of dwelling longer upon the syllabes difficult to remember than upon others, and of unwittingly glancing back in review of refractory association links. This objection is stronger nevertheless in connection with his experiments for the analysis of memorizing than with reference to studies of forgetting, because more time spent upon the less associable members of the series equalizes among all members the degrees of permanence. This quality is indeed the primary disideratum in units of measure. However, it is more satisfactory to first make the members as equal as possible, and then thoroughly to control the manner and the time of their exposure. Although, according to the results of Ebbinghaus more than one third of the forgetting of nonsense syllabes takes place during the first twenty minutes, more than one half in one hour, nearly two thirds in nine hours, and more than two thirds within twenty-four hours, only one third of his data constitutes the evidence for these four out of the seven intervals. The slow loss between nine hours and one day in comparison with that preceding and following this interval is one of the doubtful results that might have been determined with a greater degree of certainty and with small sacrifice, if the experiments had been better distributed.

We are able to improve upon the methods of Ebbinghaus, not because of the lack of value of his research; but owing to the fact that his studies have supplied a general method of investigation, and thus aroused an interest in this group of phenomena that has resulted in a fruitful series of researches, and a better understanding of the problems and difficulties to be met in this field of investigation.

While the first part of the investigation that is reported later in

TABLE II

RADOSSAWLJEWITSCH'S RESULTS

	Adults		Children	
Time	Without Meaning Per Cent. of Forgetting	With Meaning Per Cent. of Forgetting	Without Meaning Per Cent. of Forgetting	With Meaning Per Cent. of Forgetting
After 5 minutes	2.5	0.0	8.8	3.4
After 20 minutes ...	11.4	4.4	14.6	10.97
After 1 hour	29.3	19.1	22.7	23.05
After 8 hours	52.6	42.1	37.0	38.75
After 1 day	31.1	20.3	28.3	21.01
After 2 days	39.1	33.2	31.9	29.0
After 3 days	——	43.5	——	——
After 4 days	——	45.5	——	——
After 5 days	——	43.5	——	——
After 6 days	50.7	57.6	42.4	42.0
After 7 days	——	50.0	——	——
After 14 days	59.0	70.0	46.1	49.4
After 21 days	62.2	52.4	49.5	47.3
After 30 days	79.8	76.1	66.2	75.7
After 60 days	——	——	94.3	85.4
After 120 days	97.2	——	——	——

this thesis was in progress (1903–04) P. R. Radossawljewitsch was also studying forgetting. By having twenty-seven subjects, both adults and children, he avoided the individual character of the work of Ebbinghaus. He employed nonsense syllables similar to those of Ebbinghaus; but presented the syllables after the manner of Müller and Schumann, so that the manner of reading them was better controlled. The syllables were fixed on a cylinder that rotated so as to expose one syllable at a time through an opening in a screen. He used also subject matter with meaning. Each series consisted of two stanzas of Schiller's translation of "The Siege of Troy," which the reagents read throughout repeatedly until they could reproduce them twice without an error. Radossawljewitsch believed as did Müller and Pilzecker, that two repetitions gave much greater assurance of association strength than the single correct repetition of Ebbinghaus. Each of these two kinds of subject matter was relearned after various intervals, and the numbers of repetitions in learning and relearning were the measures of gain and loss. As shown by Table II. and the curves (Fig. 3), Radossawljewitsch's results indicate that we forget rapidly at first, then rather abruptly slower, and that the later rate gradually decreases. His curve of forgetting rises slower in the beginning and faster in the later portion than that of Ebbinghaus (see Fig. 3). In the cases of adults and children, with meaningless subject matter and with material with meaning, the association strength at the end of one day was greater than at the end of eight

hours or at the end of two days. In the results of Ebbinghaus there was a decrease in the rate of forgetting at about the same point. Radossawljewitsch also found a period of relatively slow forgetting between six days and twenty-one days. These later periods of retarded forgetting have no corresponding periods in Ebbinghaus's results.

Radossawljewitsch verified the results of Ebbinghaus in that long series of nonsense syllables are not forgotten as rapidly as short series. The reasons assigned for this fact are the greater amount of practise that longer series require and the enlistment of better attention by the consciousness of the relative largeness of the task.

Both adults and children were found by Radossawljewitsch to forget material without meaning more rapidly than material with meaning, especially during the first two days. His adult observers forgot 1/3 of the material with meaning in 2 days and of the subject matter without meaning in about 1 day. They lost 1/2 of the matter with meaning in 7 days, and of that without meaning in 6 days. Within 30 days they forgot 3/4 of the poetry and 4/5 of the nonsense syllables. The children lost association strength somewhat faster than the adults; but their curves for the same kinds of subject matter were quite similar.

The forgetting of connected ideas was studied by E. N. Henderson.[2] Among his subjects were school children of grades five, six and seven, high school pupils, university summer school students, college students, and graduate students. He selected as subject matter five very dissimilar passages of thoroughly coherent discourse, such as students of various degrees of development might be expected to learn and later to reproduce in class. His experiments were performed in school rooms. As much of one selection as possible was learned in three minutes, and immediately written. Two days later and four weeks later they again wrote as much of the selection as they could. The measures used were the topics, sub-topics, details and words that were memorized and that were later reproduced. He endeavored to find by means of these results the amounts forgotten within different lengths of time, the relation of age and training to the amount learned and to the amount retained, the relation of the rate of learning to the rate of forgetting, and of intelligence to forgetting. He compared also the ability to retain details with

[2] "Das Behalten und Vergessen bei Kindern und Erwachsenen nach experimentellen Untersuchungen," Leipzig, 1907.

[3] "The Study of Memory for Connected Trains of Thought," *Psy. Rev.* Monograph Suppl., Vol. V., No. 6.

TABLE III

HENDERSON'S RESULTS

	Ideas			Words		
	Gains	Per Cent. Lost in 2 Days	Per Cent. Lost in 28 Days	Gains	Per Cent. Lost in 2 Days	Per Cent. Lost in 28 Days
Summer students ...	23.1	8.8	13.6	44.2	23.3	32.3
Public school pupils	35.3	4.7	13.2	54.6	10.4	23.5
High school students	38.8	11.5	30.5	48.9	16.7	35.9
College students	39.4	25.5	31.5	53.3	39.0	47.4
Graduate students ..	43.1	23.2	36.5	58.2	32.3	49.7
Average	35.9	14.7	25.1	51.8	24.3	37.8

the permanence of larger topics. As shown by Table III. the rise in his curve of forgetting of ideas is eight times as rapid per day during the first two days as it is per day during the first month as a whole. The forgetting curve for words is nine times as fast in its rise per day during the first two days as it is per day during the first month as a whole. In both ideas and words there is a small irregular increase in the retentive ability from the younger, less mature to the older, more mature observers. Henderson believed this to be due to the greater ability on the part of the older subjects to read and to understand the subject matter rather than to any fundamental difference in memory.

Those who learn most rapidly retain a large percentage of either ideas or words; but the correlation of the two tendencies is not very high.

Two teachers made a list of their pupils in the order of their abilities, and this ranking was compared with the ranking of the same pupils in these experiments. It was found that there was almost no correspondence between these rankings among the pupils of the grammar grades. But in the higher grades there was a close relation. The most advanced reagents were superior to the less mature in grasping and in retaining general meaning. This is probably partly due to the exclusion of inferior minds from the higher classes.

The more mature students lost a larger percentage of words than of ideas. This difference was much greater with the younger, less mature subjects.

From the data of his investigation of memory for paired associates, E. L. Thorndike finds it possible to derive a few important conclusions concerning forgetting. His observers were twenty-two college seniors and graduate students. Each unit of the memory task was the ability to write the English equivalent for a given German word. One hundred or more pairs of associates of this kind were practised one hour. A new list was used each day on which practise occurred until twelve hundred pairs had been repeated.

Then the same lists were practised again. Tests were inserted from time to time. All but two subjects learned more than 90 per cent. of all the words in from 2 to 5 entire rounds. After the twenty-two subjects had practised on the average 38 hours they remembered an average of 1,030 words at the end of 3 days, and 620 words after 42 days. Tests made upon seventeen of these persons on closing the last study period and at intervals after practise show that they forgot approximately .05 within 1 hour, .10 during 3 days and from .40 to .50 by the end of 40 days.[4]

W. F. Book devotes a small portion of his most thorough-going study of learning to the investigation of the permanence of impressions.[5] He uses data from only one subject. The apparatus was a typewriter to which was attached Duprez markers which recorded (*a*) the number of letters, (*b*) the number of words printed, (*c*) the number of shiftings of the carriage, and (*d*) the number of times that is necessary for the observer to look at the keyboard. In order to discover any irregularities in the effort put forth while writing, the pulse record was taken by means of tambours adjusted to the temples of the observer. The subject practised a half hour per day 174 consecutive days with the keys of the typewriter visible. After an interval of five months he practised 60 days with the keys hidden by a screen. During the second period, his copy was a short sentence which he practised 120 times per day until he could write it 100 times per minute. The number of strokes made on the last ten days of this practise were compared with the number of strokes made in a similar manner during a ten days test, five months later, and with the number of strokes during another test, 17 months after the end of the 60 days practise. The average number of strokes per day during the last ten days practise were 1,508. The average of the first test was 1,443 and that of the second test 1,611. If these are the numbers that should be compared, there was a loss during the five months between the practise and the test, but a gain instead of a loss, as one might expect, during the twelve months between the first and second tests. Book accounts for this gain by means of the same theory that he offers in explanation of the plateaus in the curve of learning. He believes that during the 60 days of practise, in addition to the fixing of the right reactions, tendencies to many wrong reactions must have accumulated, and that these tendencies have been eliminated before the second test.

However, by careful inspection of this table it becomes evident

[4] *Pscychological Review,* XV., pp. 132–135 (1908).

[5] "The Psychology of Skill," Univ. of Montana Pub. in Psychology, Vol. I. (1908), Bul. 53, Psy. Series No. I., pp. 75–79.

that there is a gradual gain during each group of ten days, especially during the second test. Judging also from my experience with the typewriter, there is in each of these tests ample room for gain through practise. During the first test the total number of strokes was 14,424 and during the second test 16,081. This makes a grand total of 30,505 strokes after the completion of regular practise. That the gain is due to practise is even more probable in the light of the fact that the tests serve as reviews of so much practise, of the 174 days of general practise with the keyboard visible and of the 60 days of practise on the same subject matter that was repeated in the tests. Moreover, the repetitions in the tests are quite advantageously distributed for progress in learning. Further evidence for this view is found in his curve of regular practise (p. 80). The curve rises at the diminishing rate characteristic of curves of practise, finally rising between 50 days and 60 days a few more than 100 strokes. If practise had proceeded 20 days longer there is every indication that it would have gone above the point (1,611 strokes) reached by the end of the two tests. The difference between the last day's work in the regular practise (1,698 strokes), and the first day's work in the first test (1,365 strokes), indicates a loss through forgetting of 333 strokes in 5 months. Likewise the difference between the numbers of strokes of the last day in the first test (1,472 strokes) and the first day of the second test (1,390 strokes) gives a loss of 82 strokes in 12 months. There would seem also to be a loss in both these cases if, instead of using one day's work as the basis of comparison, we use the average data of three days. The first difference would be 151 strokes, the second 83 strokes. The ratio of the loss in the earlier period with the loss in the later period according to these measures agrees with most studies of forgetting.

Therefore the apparent gain that Book finds in these results can not be shown to exist except when we compare groups of ten days each. These evidences throw considerable doubt upon the need of any explanation other than that of learning, forgetting and relearning.

The problem of Ebert and Meumann[6] was to discover whether subject matter that is readily learned is forgotten more rapidly or more slowly than that which requires greater labor in memorizing. Each of five subjects memorized by the method of right associates a series of 12 and one of 16 nonsense syllables, and by the method of learning a series of successive members throughout, another series of 12 and also one of 16 nonsense syllables. Two observers learned in

[6] "Uber einige Grundfragen der Psychologie der Ubungsphanomene im Bereiche des Gedächtnisses," *Archiv für die Gesamte Psy.*, 1904, 4, 193.

addition two stanzas each of "The Siege of Troy." The material without meaning that had been learned was relearned by one observer after an interval of 75 days, by a second after 85 days, by a third after 91 days, by a fourth after 146 days, and by a fifth after 156 days. The matter with meaning was relearned by one subject after 146 days and by the other after 156 days. I have condensed Ebert and Meumann's results for meaningless subject matter into Table IV. The most evident facts about these results is that by the use of one of these methods more of the easier series was forgotten, and by the other method more of the harder series was forgotten. Therefore the method of right associates seems to be better adapted to the sixteen syllable series than to the twelve syllable series. With the other method the reverse appears to be true. It may be noted also that there is better correlation between the length of the interval of delayed recall and the amount forgotten. This is doubtless due to the fact that, not only the series, but also the observers were not the same for any two intervals. The subject matter with meaning learned by the consecutive method was more easily learned than nonsense syllables. As in the case of subject matter without meaning, no forgetting occurred, and the observer with the shorter of the two periods was 25 per cent. more able to relearn a stanza at the end of the interval of delay than at the termination of practise.

TABLE IV

EBERT AND MEUMANN'S RESULTS

	Method of Right Associates						Method of Consecutive Members					
	12 Syllables			16 Syllables			12 Syllables			16 Syllables		
Intervals	Learning	Relearn	Per Cent. Loss	Learn	Relearn	Per Cent. Loss	Learn	Relearn	Per Cent. Loss	Learn	Relearn	Per Cent. Loss
75 days	8.0	5.0	37.5	8.0	6.5	18.8	6.0	6.0	0.	8.0	7.0	12.5
85 days	4.0	3.5	12.5	7.0	7.0	0.	7.0	7.0	0.	10.0	7.0	30.0
91 days	12.5	7.5	40.0	7.5	7.5	0.	11.0	10.0	9.1	18.0	12.0	33.3
146 days	1.5	2.0	—33.3	4.0	4.0	0.	4.0	4.0	0.	5.0	4.0	20.0
156 days	8.0	7.5	6.3	5.5	5.5	0.	5.0	5.0	0.	5.0	6.0	—20.0
Gross Av.	6.8	5.1		6.5	6.1		6.6	6.5		9.2	7.2	
Per cent. Av.			12.6			3.8			1.8			15.2

An experimental study of the curve of forgetting was made also with pathological subjects by Ziehen.[7] He used this type of observers because one is justified by the principles of psychopathology in expecting that the ability of most patients to recall what they have

[7] "Das Gedächtnis," Berlin, 1908.

learned when they are at successive stages of disease should correspond to the ability of normal persons to recall at different stages of forgetting. He says, "Investigations have shown that loss of memory images is quite slow for a considerably extended time (ziemlich beträchtlichen Zeit), and then there is exceedingly rapid loss." This "critical point" of the curve has a position dependent upon the disposition of the learner, the kind of incentive, and other factors.

He states that it has been experimentally demonstrated by others and by himself that, from this critical point on, the recent is forgotten more rapidly than the more remote by most of those having memory defects. The few exceptions are in special types of insanity. Ziehen's explanation for this phenomenon is that we recall nothing unless its reproduction is necessitated by some experience that precedes it; and as the old experience has become consciously and subconsciously, or cortically, more connected during the intervening time than has been possible for the recent experience, this new experience is more difficult to recall until it has had time to thus become a more intimate part of the individual.

Moreover Ziehen finds that forgetting is not all passive, but that holes are torn in the memory image, and also older images are modified by the play of newer associations in normal individuals similar to the way in which they are distorted in pathological amnesia.

In none of the other investigations that I have been able to find does the curve of forgetting have the form described by Ziehen. The nearest approach to it is the curve found by Radossawljewitsch, which rises slowly during the first five minutes, and then much more rapidly. I believe that the solution of problems in arithmetic, which he used as one form of subject matter, usually depends largely upon a fundamental principle and a group of related, subordinate principles. When the fundamental principle finally passes below the threshold of recall, the dependent principles would thereby lose suddenly a large part of their association strength. But this explanation could scarcely be applied to his other subject matter, six, eight and nine place numbers. Ziehen's description of his method and results is not complete enough to permit much use to be made of his conclusions.

Several more recent investigations of kindred problems have led to conclusions concerning forgetting. The following declaration is made by A. Renda:[8] "Forgetting is not merely an accidental characteristic of mental function, but is the result of an active process of dissociation. It is a means by which consciousness gets rid of re-

[8] "L'oblio saggio sull' attivita selettiva della coscienza," Torino, Bocca, 1910, p. 229.

dundant material." Pieron[9] says that forgetting is like auto-catalysis in chemistry in that it is a change that ensues in a memory image by reason of the presence of transforming agencies in the image itself. These views have evidences, but no counter-evidences in the results of the other investigations previously quoted.

The results of Ebbinghaus and Radossawljewitsch resemble each other in that the rate of forgetting is more rapid at first than later; but they disagree in the amount of difference between the early and the late portions, in the degree of abruptness in the change, and in the other irregularities in the curve. Ziehen's curve seems to be radically different from all the others. Several of the investigators have found data for too few intervals to furnish much assistance in harmonizing these curves until there is additional investigation in the same field.

[9] "L'evolution de la memoir," Paris, 1910.

CHAPTER II

The Problem and the Method

Investigation has revealed the fact that two kinds of results arise from the insertion of intervals into an act of learning. If recall is delayed, the association links become weaker. If practise is interspersed with periods of relative inactivity, the repetitions have greater value. The most advantageous degree of the distribution of practise through the interjection of periods of rest has been theoretically but not experimentally determined. Nor is the cause of this phenomenon much more than a question under discussion. Even the practise curve has been determined only as to its general form, and there is disagreement concerning its details. Moreover in the case of forgetting, as has been shown by the previous chapter, not only the details, but also the general trend of the curve is in dispute.

The results of investigation make it probable that we forget faster immediately after practise than later. The earlier part of Radossawljewitsch's curve rises more slowly and the latter part more rapidly, however, than that of Ebbinghaus, and has several stages of slow forgetting that are not present in the curve of Ebbinghaus. Explanations of these differences have been either theoretically proposed or are merely foreshadowed by the results.

This thesis is an endeavor to study the same field with several kinds of subject matter and various methods. The fundamental problem is whether all forgetting is governed by the same general law. The secondary aim is to discover, if possible, why the results of various investigations do not more fully agree.

The first group of experiments reported in this thesis was performed in 1903 in the University of Chicago, and was discontinued after five months because of the demands of a new position. In the mean time Radossawljewitsch began his investigation of practically the same problem. His work was published in 1904. The second group of experiments of this thesis was performed with entire classes in the Indiana State Normal School in Terre Haute, and by reason of its nature, it covered a long period (1904–09). The last three groups were performed under the direction of the department of psychology at Columbia University during 1909 and 1910.

Previous researches have shown incidentally that learning and forgetting take unlike forms and progress at varying rates with the

slightest differences in subject matter and in types of learners, with relatively trivial deviations in method, with changes in health or in time of day, as well as with an increase or a decrease in the periods between practise and recall. However, both phenomena have shown constant relations between controlled conditions and consistent results. Therefore, the choice of subject matter and of methods has been with this end primarily in view. It should be possible to isolate a factor of this nature for study without departing from the conditions of learning in daily life including those of school. Facts are no less scientific when practical. The derivation of principles from that source paves the way for their most convincing verification through successful application.

As simple subject matter without meaning can be learned easiest and most rapidly by the so-called "trial and success method" and complex subject matter with meaning by more rational methods, there is likely to be some difference in the manner in which they are forgotten. The content to be learned will, then, be simple and complex material similar to that of practical life.

As far as possible such subject matter was chosen as would appeal to but one sense department, and would not be a stronger stimulus to persons of one age, sex or race than to those of another age, sex, or race. In the first group of experiments the subject matter was a series consisting of one muscular movement repeated many times. In the second group a number of sensations constituted the series. The subject matter in the third group was complex. Each member of the series of stimuli was responded to by a corresponding member of a series of movements.

The methods were also made to correspond as far as possible with those which are most serviceable in daily life. All conditions were kept constant except the intervals between practise and recall. The short periods that were necessary for the making of records and to readjust apparatus were timed carefully so as not to allow any differences in the association strength or in the values of new repetitions owing to variations in the distribution. Sufficient control was exercised to make the results comparable; nevertheless, the limitations were so few that after the experiments were well begun, the reagents were unconscious of restraint. The stimuli were presented to the most used sense, vision, and were responded to by movements. Series were practised as a whole, as in everyday life rather than piecemeal as is too often done in school. The observers were all adults with no very individual tendencies. The details of method in the individual experiments will be discussed in their particular chapters.

CHAPTER III

The Forgetting of a Relatively Simple Motor Act

The experimental work in this chapter was performed in the University of Chicago during 1903–04. Although the data can be of little value in the determination of the curve of forgetting they throw some light on the conditions of learning. The earliest plan was to begin the study of forgetting by using a series consisting of the repetition of one act in which the motor element is highly dominant. Throwing was chosen as being a distinctly human acquisition that is probably not based on an innate tendency as such, and that is typical of common forms of manual skill.

It soon became evident that it is more difficult to secure accurate scores in throwing than it is in other kinds of target practise. In an effort to obtain reliable scores, the target was made to undergo several transformations. The final and most successful target consisted of hollow cylinders made of wire mosquito netting of diameters corresponding to the diameters of the circles used in the targets of most of these experiments. These cylinders were closed at one end, and strengthened with a ring of stiff wire at the open end. They were placed within each other in the order of size, and supported so that the curved wall of one was equidistant from the corresponding walls of the others. The open end of this compound cylinder was placed toward the thrower so that one and three fourths inch wooden balls, when thrown, would pass into one or another of the cylinders according to the accuracy of the throws. When a projectile struck the edge of a cylinder, it fell into the next larger one, because each cylinder was shorter than the next larger, and adjusted so that its open end was five inches further from the thrower than that of the next larger cylinder. The entire target was mounted so that a projectile rolled toward the closed end of the cylinder into which it had been thrown and into a trough-like crease or groove in the lower side of the cylinder where it took its place in line with others that scored equally with it. Along each crease was read directly the score made by lodging any specific number of projectiles in that division of the target during a series of one hundred throws. After having read the scores, a small metal plate, that covered an opening opposite the rear of each groove, was swung to one side to allow the balls to roll out of the target in preparation for another series.

It was readily ascertained by preliminary experiments that, when other conditions were constant, series thrown by the right hand, by the left hand, in the under-hand, in the over-hand, and in the direct modes did not improve at parallel rates with equivalent amounts of practise, nor did they decline equally during the same interval. Successive series with one hand and with one kind of throwing movement were adopted as the most nearly comparable.

One reagent was a university professor, and ten other observers including the writer were graduate students in psychology. Each reagent was free to throw in a manner of his own choice except that he was required to continue throwing in the same manner throughout a group of experiments. The throws were made to coincide with the strokes of a metronome bell. Further preliminary experiments were performed to find the rate of throwing that yielded the greatest progress. Five persons were found to have individual rates at which throws could most advantageously succeed each other. These rates were always as rapid as projectiles could readily be picked up and thrown. Later each of two reagents performed one thousand throws with each of the following intervals. There are too few subjects in

TABLE V

The Value of Intervals during Practise

	30.0 Sec.	60.0 Sec.	120.0 Sec.	180.0 Sec.
Per cents. gained	49.2	24.9	6.3	6.7
Per cents. gained	57.8	14.3	23.2	10.0
Average gained	53.5	19.6	14.8	8.4

this preliminary test, to serve as conclusive evidence that with this kind of material, the shorter the interval the more rapid is the progress in learning; but these evidences indicate a likelihood that Bergström's principle, "the acquisition and retention of associable words varies approximately as the interval between the members of a series," may apply only to complex subject matter with meaning. In relatively meaningless matter, for example in throwing, the formation of a series out of the mere repetition of an act, the progress seems to vary approximately inversely as the number of repetitions within a given time. The only limits to this in throwing were that the speed should not exclude the accurate picking up of projectiles and the aiming and proper swinging of the arm in throwing, and the avoidance of fatigue. This would not warrant the division of subject matter into two distinct classes to be learned in two very different ways; but it would mean that the intervals between members of series and between entire series should depend upon the degree of complexity of the series. Ebbinghaus states

that the more repetitions of nonsense syllables he could crowd into a given time, the quicker he could learn them. Lottie Steffens found that with her subjects rapid learning was not necessarily conditioned by many repetitions within a short time. Moreover it has been shown that with simple subject matter the trial and success method, as it is called, produces quicker, surer results, whereas it has been repeatedly demonstrated that subject matter with meaning is learned more economically by thinking one's way through at first. Because of the results gained in these tests, the throwing was done as rapidly and as persistently throughout each period as the picking up of projectiles and the avoidance of fatigue would permit. Other preliminary tests made it evident that a target to be thrown at by inexperienced men and women at a distance of six meters, should be about a meter in diameter. It also became evident that the projectiles should, if possible, be adapted to the size of the thrower's hand; but that an average size would be about one and three fourth inches in diameter. Slight inequalities in the weights of projectiles used resulted in relatively great deviations in scores. The general effect of practise made the point at which the practise of each series began a variable quantity. The influence of this defect was reduced somewhat by finding what per cent. of the gain was lost through delay.

In the principal experiment of this group such meager gains were made in throwing one hundred times that the loss due to delayed recall could not be measured with any degree of certainty. Persistent throwing several hours per day during a series of days yielded more commensurate gains; but the decrease in association strength was proportionately slow. In order to find whether the small scores were due to a defect in the method or to the inability of the psycho-physical organism to habituate itself rapidly to this kind of reaction, the apparatus was so modified as to record the force as well as the direction of each throw. Back of each corner of a square wooden target of one meter side was fixed a large tambour with its three inch rubber membrane on the side towards the wall, against which the target was suspended by ropes. A screw was driven into the wall in such a way that its head, which was two inches in diameter, rested against the middle portion of the membrane. The air chambers of these tambours were connected by means of tubes with one tube which led to a Marey tambour. When a projectile hit the target, the momentum with which it struck was recorded on a revolving kymograph drum by the pen of the Marey tambour. The throws were at first very unequal in force, but they rapidly and regularly became equal as practise proceeded, and with an equal degree of regularity they became unequal in intensity with longer and

longer intervals of forgetting (Fig. 1). Therefore much more progress was being made in learning to throw than was evident in terms of control of the direction of the throwing movements. As the increase or the decrease in the amount of energy expended is more immediate and more simple than the guidance of movement, its mastery is probably more easily acquired in games and in work.

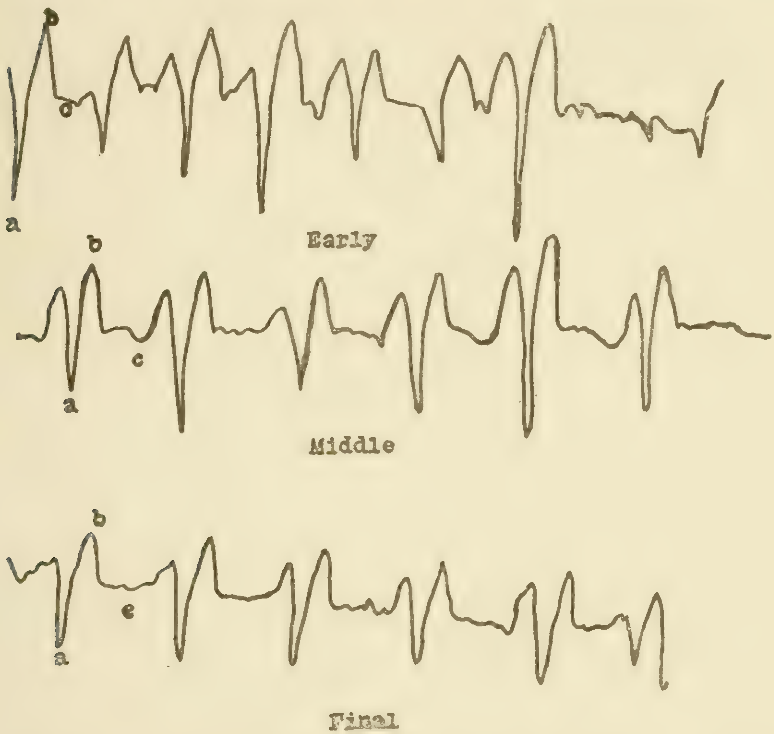


FIG. 1. Pantographic copies of records of the force of throws. Points *a* were made by the hitting of the target. The record of the greatest recoil and of the lesser recoils are indicated by *b* and *c* respectively. The "early" curves were made near the beginning of a seris of 100 throws and the "final" curves were near the end of that series.

The complexity of the control of movement in three-dimensional space was evident in the fact that the spots that recorded the "hits" were arranged in the form of an ellipse with its long axis parallel with the motion of the arm. This axis shortened much faster than the other axis as skill increased, and was relatively short with persons who were accustomed to throwing in the manner used. Skill in throwing is therefore largely ability to release a projectile at the proper moment. All of the reagents reported that they found them-

selves strenuously endeavoring, while throwing, to avoid "wild" throws, that is, "hits" beyond their usual limits, and also were conscious of trying to strike the bull's-eye. Sometimes one of these purposes and sometimes the other was uppermost in mind. Total scores showed later that near the boundary of each reagent's field, was a ring-like area in which the "hits" were more numerous than in closely neighboring portions. In the region of the bull's-eye there was a somewhat similar sudden increase in the number of "hits." "Wild" throwing was usually due to the momentary straying of the attention or to loss of confidence owing to previous "wild" throws. After about fifty throws of a practise series, the score almost always decreased, and the "wild" throws increased unless the bull's-eye was varied in color or in some other quality to overcome this inconstancy of attention. The necessity of this renewal of the stimulus was probably due to the monotony of a series that was merely the repetition of one sensori-motor act.

The larger the number of muscles involved in a psycho-physical act, the greater is the number and variety of possible reactions, and therefore the slower is the progress. The rate of learning is more rapid in the use of the preferred hand than in the use of the other hand.

In so far as the results of this group of experiments have value, they indicate that the conditions of progress in the study of forgetting are: a series consisting of varied members preferably with meaning, rather than a series composed of repetitions of one short interval between simple reactions and of longer intervals the more complex the reactions; attention kept at its highest intensity, if necessary, by means of devices; as small a group of muscles as possible, muscles previously coordinated in other acts but not in the one that is being practised.

CHAPTER IV

The Forgetting of Sensations with Relatively Little Meaning

After having spent several months in the almost fruitless efforts to modify the methods and apparatus of the experiment described in the previous chapter, I endeavored to retrieve the loss by performing a group of experiments during spare moments while teaching. This work is a study (1) of how meaningless, relatively simple subject matter decreases in association strength, (2) of the manner in which students often forget what they learn in class, and (3) of how students recall under class-room conditions.

The experiments were performed with entire classes of students in the Indiana State Normal School. These students had largely attained the degree of physical and mental maturity of college sophomores. Nearly one third of them were men, and more than two thirds were women.

The subject matter was a consecutive series of nine consonants that had been found by experiment to be about equally difficult to remember. Vowels were omitted in order to avoid the possibility of combining members of a series into syllables. Consonants were chosen rather than numerals; because consonants furnish an opportunity for recall not only by reproduction but also by identification; whereas the selection of nine digits from among the few would not be a fair test of recall. Most persons are habituated in the use of numbers, and even of letters in calculation, and are familiar with the employment of letters as initials and symbols without definite reference to what they symbolize; but, as a rule, they do not use words and syllables in that manner. Therefore, letters have not such a strong tendency as words or syllables to suggest meaning. For these reasons a series consisting of letters was chosen as a type of meaningless content. These letters were presented successively, and each series was repeated as a whole. Each consonant in the series was presented one second with no perceptible interval between the presentations. The letters were one and one half inches in height, and were arranged at intervals of three inches in a vertical column on a sliding screen. In front of the letters was a stationary screen with an opening through which one of the letters at a time could be seen. On the back of the sliding screen opposite each letter a small roller was fixed with its axis perpendicular to the surface of the screen.

These rollers were placed alternatingly on two parallel lines which were drawn vertically along the middle of the sliding screen. Behind this screen a pendulum was suspended at its middle point. This pendulum carried a bob at each end. Attached to the front of the pendulum rod was a metal half ring with a radius of about two inches. Its concave side was upward and its center coincident with the point of support of the pendulum. It was so placed that one of the rollers resting upon it supported the sliding screen. When owing to the swinging of the pendulum, this roller passed through an opening at the middle of the half ring, it allowed the screen to fall until the next roller was caught by the half ring. One second later it, in turn, fell through the opening. It is evident that while a roller rested on the half ring, one of the letters was being exposed through the slit. The half ring was padded at places where the rollers first struck, in order to diminish the noise resulting from the impact. Three additional rollers permitted the swings of the pendulum to become regular before the exposures began. As one series was used for all intervals of delayed recall each group of students served for only one interval. Preliminary experiments indicated that this plan would be better than that of using different series and of having each class serve for several intervals. Besides, as the Indiana State Normal School is not an institution of research, I preferred not to use much time for an experiment after it ceased to be a profitable exercise for the students. However the whole quantity of results shows that it would have been better to have experimented with several series with the same subjects.

It was not a totally new experience for the students in these classes to take part in psychological experiments; but there was enough unusualness about it to increase the tendency to vocalize the letters. They were therefore urged to abstain from moving the vocal organs. This reduced the action to about its normal degree. It was made clear that if any one failed to do honest work, he would gain nothing, and that the science of psychology would probably lose. Even those did faithful work who were known by their teachers to need watching in examinations. The mode of procedure was as follows: Each reagent was given a slip of paper bearing a number. After the presentation of the entire series the reagents of one group were allowed two minutes during which to write the letters that could be remembered, and as far as possible to place them in the order in which they had been presented. They then folded the papers in each case so as to conceal during the remainder of the experiment, the letters which they had just written. In this manner, the presentation of the same series, the writing, and the folding were

performed six times; but any reagent who was moderately sure before any presentation that he had correctly written the series withdrew from the experiment by occupying himself with some other interest or duty. When all had finished, they recorded their physical condition as good, medium, or poor. The papers were then collected.

After a period of from one to twenty-eight days, each subject was supplied with paper on which he placed the number that he had found on the paper given him when the experiment was begun. This made it possible for me to compare his two papers. He then reproduced and recorded the letters and their order in so far as he was able. To this he added the information that was necessary in determining the value of his results. One item was the condition of his health. Another was whether any part of the series had been recalled during the interval, and whether he had been able to crowd it out by turning his thoughts to more interesting matter. He stated also whether he had associated the letters with initials or words and whether he had used rhythm or had vocalized.

Another group of subjects were allowed one and a half minutes to select the letters of the series from among the eighteen consonants of the alphabet and a half minute in which to arrange in the original order the nine consonants of the series, which were shown them in mixed order. Thus were obtained data for the comparison of the methods of selection, reproduction and reconstruction.

The tables do not include the results (*a*) of students who were present for only one part of an experiment, (*b*) of the few who did not understand how the experiments were to be performed, (*c*) of the subjects who recalled more than one letter of the series during the interval of forgetting, (*d*) of the reagents who associated some of the letters with initials or other subject matter, (*e*) of one observer who was not strictly honest in her work, and of several who were accidentally aided in reproduction, (*f*) of reagents whose health conditions were not normal or whose sight was too defective to produce reliable results. After these had been dropped, the results of 756 subjects constituted the Table VI., and of 348 constituted Table VII. The subject matter that could not be recalled after intervals of one to twenty-eight days is shown in terms of reproduction in Table VI., and in terms of recognition and reconstruction in Table VII. In both tables each gross measure is the average of the number of errors after the interval of delay opposite which the measure stands. The per cent. of the given subject matter that was forgotten is in a companion column. As there were nine letters in each series, nine omissions were possible. Therefore each omission was valued at 1/9 of 100 per cent. It was expected that insertions

would occur only when opportunities for insertion were made through omissions; but inspection and calculation show that there is no correlation between the omissions and the insertions. Hence each letter inserted was also valued at $\frac{1}{9}$ of 100 per cent. There is no general agreement concerning the methods of evaluating errors of order. Every plan is based upon a theory of the manner in which members are associated in a series. In the case of the series of letters whenever one member was one removed from its given place in the series there were on an average as many evidences of weakness in the association links as when one member was from two to eight removes from its proper locality. Therefore in this group of experiments one letter out of place was counted one error of order regardless of the number of letters between it and its proper locality.

TABLE VI

THE METHOD OF REPRODUCTION

	Omissions		Insertions		Errors of Order		Totals			
Intervals	Gross	Per Cent. of 9 Errors	Gross	Per Cent. of 9 Errors	Gross	Per Cent. of 9 Errors	Gross	Per Cent. of 27 Errors	Av. Per Cent. Per Day	Observers
1 day	1.1	12.2	0.5	5.6	1.4	15.6	3.0	11.1	11.1	41
4 days	1.1	12.2	0.6	6.7	2.4	26.7	4.1	15.2	3.8	114
7 days	1.6	17.8	0.6	6.7	3.1	34.4	5.3	19.6	2.3	140
14 days	2.6	28.9	0.9	10.0	2.3	25.6	5.8	21.5	1.5	298
21 days	2.2	24.4	0.9	10.0	2.6	28.9	5.7	21.1	1.0	123
28 days	2.0	22.2	1.3	14.4	2.4	26.7	5.7	21.1	0.8	40

TABLE VII

THE METHODS OF SELECTION AND RECONSTRUCTION

	Selection				Reconstruction				
	Omissions		Insertions		Errors of Order		Totals		
Intervals	Gross	Per Cent. of 9 Errors	Gross	Per Cent. of 9 Errors	Gross	Per Cent. of 9 Errors	Gross	Per Cent. of 27 Errors	Observers
1 day	0.7	7.8	0.4	4.4	1.9	21.1	3.0	11.1	21
4 days	0.9	10.0	0.4	4.4	2.9	32.2	4.2	15.5	100
7 days	1.3	14.4	0.4	4.4	3.7	41.1	5.4	20.0	40
14 days	1.8	20.0	0.6	6.7	2.8	31.1	5.2	19.3	55
21 days	1.6	17.8	0.7	7.7	3.1	34.4	5.4	20.0	92
28 days	1.9	21.1	1.3	14.4	2.9	32.2	6.1	22.6	40

Table VI. contains the data obtained by the method of reproduction. Table VII. consists of results of the methods of selection and of reconstruction. Table VII. was produced in one group of ex-

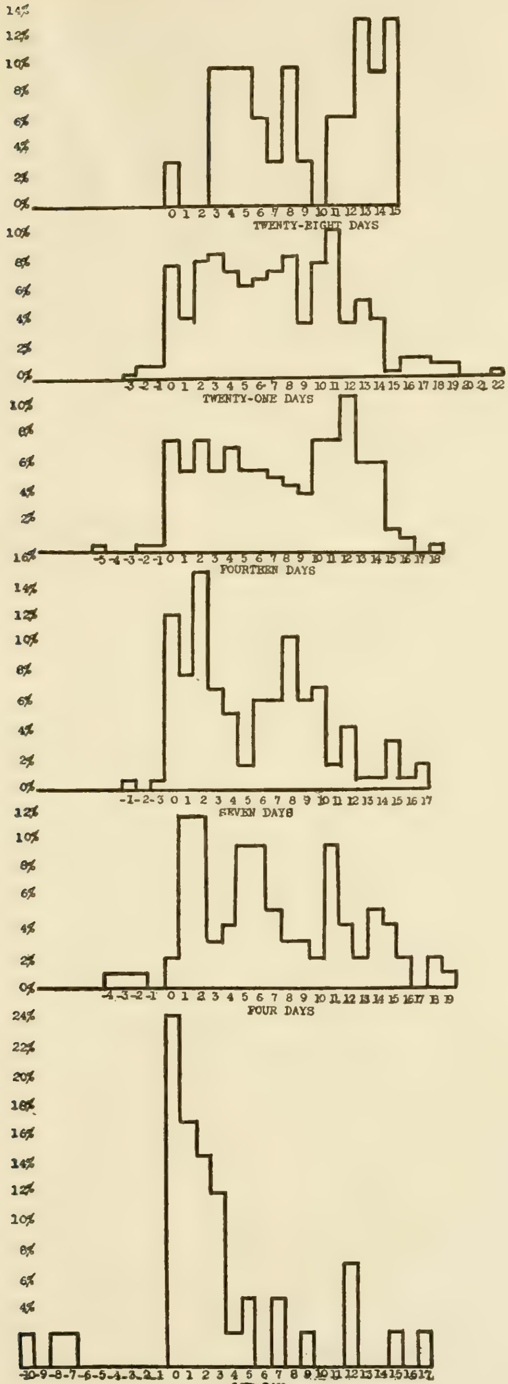


FIG. 2. Surfaces of distribution.

periments and is therefore not given as two separate tables. In both Tables VI. and VII. the total per cent. of errors is not, like the total gross errors, the sum of the errors in the three preceding columns; but it is the per cent. of the total twenty-seven errors that are possible in each experiment.

The errors of Table VI. increase rapidly during the first fourteen days. During the remainder of the twenty-eight days no additional errors are evident in the results. According to the method of reproduction, therefore, 1/3 of the gain is lost by the end of the first day, and 2/3 within 14 days. By the method of selection also the errors are found to increase rapidly for 14 days with few additional errors thereafter; but the errors of the insertion of letters increase rather gradually to the end of the period studied. Seven days is the limit of rapid forgetting according to the method of reconstruction. The loss of association strength after seven days is rather slow. The increase in errors of insertion is greater than in errors of omission, and in errors of order.

The surfaces of distribution (Fig. 2) were constructed from the errors in the method of reproduction and therefore correspond to Table VI. As the numbers of cases reported in connection with the six intervals are quite unequal, each surface is made on the basis of percentage of error. There would be no cases, of course, with less than zero errors, were it not that for unaccountable reasons, a few individuals made more errors at the close of practise than during the test. A large percentage of individuals made no errors when the interval was one day; and this percentage decreased with the lengthening of the interval, as shown by the mode that was at zero when the intervals were shortest, and that arose to the larger measures as the intervals lengthened. But there seems to be another mode that fluctuates somewhat within the range 8 to 12 errors, and has a tendency towards larger measures as the intervals lengthen. This indicates that the subjects who participated in these experiments may have belonged to two distinct groups. Most of the members of one group made from zero to 4 errors, and those of the other group from 8 to 12 errors when the delays are from 1 to 28 days. If under some circumstances the cause of one of these modes, and under other circumstances the cause of the other is dominant, considerable differences in the curve of forgetting, such as show themselves between the results of different investigators, might easily arise. Therefore I endeavored to find two or more causes of loss in association strength. The most profitable ones are two or more groups of reagents who do not react alike under a given set of conditions. The mode of reaction must necessarily be one that increases the number of errors of

one of the types of observers and either leaves the errors of the other type unchanged, or decreases them, or increases them at a different rate. Even though all reagents had been instructed to practise no more after they were moderately sure that they had written all the letters in their correct order once, only 24 per cent. of them ceased at the stage of practise desired; whereas 39 per cent. exceeded this limit, on the average, by a little more than $1\frac{1}{2}$ repetitions, and the remaining 37 per cent. believed that they had reproduced the letters in their proper order before they had done so. The data of these three groups were made into separate tables and into corresponding surfaces of distribution with the hope of thus separating the two or more types that are responsible for the existence of more than one mode. But the general form of surfaces representing the segregated groups bore the bi-modal character of the composite surfaces.

Guided by numerous clues of the above type, a group of experiments was performed in order to discover the cause of this bi-modality. The observers were Normal School students, distributed by influences that had nothing to do with their individual abilities into three sections of a class in psychology. The method of experimentation was similar in many respects to that of the other group described in this chapter. Each paper was made to indicate whether the reagent was a man or a woman, white or colored, and in good, medium, or poor physical condition on each day of the experiments. The studies being pursued, and an accurate estimate of the average number of hours and fractions of hours per week spent in study were also reported. The interval was seven days in all experiments in this group. The reagents in section *A* were allowed to have the impression that the experiment was complete at the end of practise. Sections *B* and *C* were told that they would be required to reproduce the series later, and that they should therefore avoid discussing the experiment and should have a more interesting subject matter ready to absorb the attention whenever the series entered the mind. Section *C* was instructed to avoid the use of devices in learning. Section *B* was directed to use rhythm with three members of a series to the measure. No mention of devices was made to section *A* until practise had been completed. After practise and after recall, subjects of all sections recorded their introspections regarding the use of rhythm, association, and other aids.

Tables VIII. and IX. make possible a general comparison of sections *A*, *B*, and *C*. The expectation of the final test did not decrease the number of errors made by the members of section *C*, as they made a larger number of mistakes than the observers in section *A*, none of whom knew that this test was to be given. The difference between

the results of these two sections may have been due, however, to the other differences in the method of conducting the experiment, the permission and prohibition of devices. There is evident a secondary mode in the neighborhood of 4 in the total distributions, and also in all partial distributions of sections *A*, *B*, and *C*, that deviate sufficiently from the primary mode near zero and one. The cause of the bimodality that we are studying in this group of experiments is not evident therefore, in any factor that is shown in Tables VIII. or IX.

TABLE VIII

Central Tendencies and Deviations

	Omission			Insertion			Order			Totals		
	A	*B*	*C*	*A*	*B*	*C*	*A*	*B*	*C*	*A*	*B*	*C*
Averages	0.91	0.4	1.08	0.25	0.2	0.3	1.75	0.8	1.65	2.92	1.4	3.04
Medians	0.79	0.32	0.63	0.38	0.42	0.26	0.13	0.17	1.0	3.3	0.62	0.83
Modes	0	0	0–1	0	0	0	1.0	0	0	1.0?	0?	0–1?
Av. Dev.	0.94	0.59	0.81	0.44	0.35	0.42	2.02	0.96	1.49	1.79	1.65	2.4
Probable Errors										1.04	0.9	1.92

Both groups of experiments described in this chapter were performed during a prosperous industrial period. As is usual under such circumstances, fewer of the most capable young men were attracted to the schools in which preparation is made for the ill-paid vocation of the teacher. Consequently, in general, the women in the Indiana State Normal School were doing school work of a somewhat higher grade than was being done by the men. Men and women were, therefore, two groups that might forget at different rates. As

TABLE IX

The Sexes and Forgetting

	Men		Women	
	No.	Errors	No.	Errors
Group *A*	3	6	29	93
Group *B*	2	7	13	14
Group *C*	2	7	21	63
	7	20	63	170
		Av. = 2.86		Av. = 2.89
				Dif. = 0.03

the larger number of errors would be expected to appear in the results of the students whose class grades were the lower, and as about one student among three was a man, it seemed probable that the men's errors constituted the secondary mode. However, as shown in Table IX., the women made nearly the same per cent. of errors as the men. Therefore the two modes are not due to the difference in sex.

There were not enough colored reagents to have as great an influence as was shown by the surfaces of distribution.

Sixty-nine per cent. of group *A* and 38 per cent. of group *C* used rhythm. Table X. shows that these observers retained 16.12 per cent. more of what they had learned than was retained by members of the same groups who did not use rhythm. Group *B*, all of whom used rhythm as directed, made 57.23 per cent. fewer errors than those who were able to avoid rhythm in group *C*. However, if these facts account in any measure for the bimodality that is found in the results of this chapter they can not be the sole cause; for the distribution of group *B*, in which all rhythmized, contains as much evidence of bimodality as that of groups *A* and C (Table VIII.). Rhythm reduced omissions about ⅔ in number, insertions ⅓ and errors of order ½. Therefore it prevents omissions especially and aids much in retaining the order of a series. It is possible that this would throw the errors into two groups.

TABLE X

THE USE OF RHYTHM

	Rhythm				No Rhythm			
	Omissions	Insertions	Order	Totals	Omissions	Insertions	Ordor	Totals
Group *A* ...	1.07	0.25	1.6	3.0	1.00	0.83	2.33	4.2
Group *B* ...	0.33	0.1	0.9	1.33				
Group *C* ...	0.93	0.33	1.93	3.13	1.33	0.22	1.56	3.11
A and *C* ...	2.00	0.58	3.53	6.13	2.33	1.05	3.89	7.31
Average	1.00	0.29	1.77	3.07	1.17	0.53	1.94	3.66
				Differences =	.17	.24	.17	.59

Gain through rhythm .... 16.12 per cent.

In order to find whether the two modes could be due to differences in sensory type, an effort was made to determine in a general way whether the subjects were predominantly visual or auditory. Twenty numbers of three digits each were alternately either pronounced or exposed to their view. The period of presentation, which extended from the end of a pronunciation to the beginning of the next pronunciation or from the end of an exposure to the beginning of the next exposure were equalized by the means of a metronome. The observers were instructed to keep the sound of each spoken number in mind throughout its period and to look at each visible number during its entire period. Each period was four seconds in length. After the presentation of the whole series, all the numbers that could be remembered were written in one column. The number of members of the visual and of the auditory presentations that were written correctly determined the type. Only those

whom the results designated as pronouncedly of the one type or of the other were included in Table XI. An inspection of the table will show that there is no relation between visualizers and audiles that indicates that either type learns much faster than the other. But when both avoid rhythm (sec. *C*) the visualizers forget 150 per cent. more than do the audiles. When both use rhythm (sec. *B*), the auditory type loses 350 per cent. more than the visualizers. These

TABLE XI

SENSORY TYPE

	Auditory No.	Auditory Errors	Auditory Av. Errors	Visual No.	Visual Errors	Visual Av. Errors
Group *A*	4	17	4.2	23	71	3.1
Group *B*	4	7	1.8	5	2	.4
Group *C*	6	12	2.0	7	36	5.1

reactions unite in the verdict that both are influenced by rhythm but that audiles depend more upon rhythm than do visualizers. This is in agreement with those theories of rhythm which assign a larger place to hearing than to sight as a means of acquisition of the rhythmic tendencies of consciousness. Table XII. shows the rela-

TABLE XII

RATE OF LEARNING

	Rapid No.	Rapid Errors	Rapid Av. Errors	Medium No.	Medium Errors	Medium Av. Errors	Slow No.	Slow Errors	Slow Av. Errors
Group *A*	20	58	2.6	7	24	3.4	5	17	3.4
Group *B*	7	3	0.4	4	2	0.5	4	16	4.0
Group *C*	9	27	3.0	7	23	3.3	6	19	3.2
	36	88		18	49		15	52	
	Average		2.44	Average		2.72	Average		3.47
	A.D.		1.7	A.D.		2.3	A.D.		2.0
	P.E.		.04	P.E.		1.7	P.E.		1.7

tions that were found between the rate of learning and the rate of forgetting. In collating this table, individuals were classed as slow, medium or fast on the basis of the repetitions that were necessary in learning a series. This classification corresponded quite closely with the time spent in the preparation of lessons and with my knowledge of the kind of work done by each student for other teachers as well as for myself.

The table shows that the rapidity of learning and the rapidity of forgetting were in inverse ratio with each other. The slow learners forgot 42.3 per cent. more during the seven days than the rapid learners. The average deviations and probable errors of the losses

in association strength were much smaller in the case of the rapid learners than in case of the slow learners. Hence rapid learners are more alike in their manner of forgetting than are slow learners. This is no doubt due to the greater regularity and smaller number of wrong reactions which are the essentials of speed. All gained through the use of rhythm except the slow learners. However, two modes are evident in nearly all of these distributions.

By means of rhythmical repetition (sec. *B*), the rapid learners decrease their errors due to forgetting 87 per cent.; the medium learners also diminish their errors nearly 85 per cent.; but the slow learners lose 25 per cent. more. These variations do not make a gradual transition from large through medium to slight change in rate of forgetting; but we can state with some degree of assurance that rhythm aids rapid learners, and hinders slow ones.

There were differences in the rates of forgetting, however, between rhythmizers and non-rhythmizers, between audiles and visualizers, and between rapid and slow learners. Moreover, when visualizers and rapid learners used rhythm, there were decided diminutions in their errors; but when they avoided rhythm, they increased their errors; and when audiles used rhythm, there was also a slight falling off of errors; but when slow learners used rhythm, the errors were increased about 1/3. According to these results the two groups of observers could be either rhythmizers and non-rhythmizers, or audiles and visualizers, or slow learners and rapid learners, were it not that in each case a secondary mode is apparent in the neighborhood of 4 errors. This appearance of a secondary mode whenever distributions extended from zero to 4, together with the appearance of a similar bimodality in the distribution of Ebbinghaus for 2 days, 6 days, and 31 days (see Table I.), and also my failure in grouping the measures so as to find the cause in too few measures, are indications of a difference in results that is worthy of further investigation.

CHAPTER V

Forgetting of Sensori-motor Experiences

In previous groups of experiments the investigations have been made with simple subject matter. In one case, the series consisted of a single motor reaction repeated a number of times. In another case, the members of the series were varied. Throwing was chosen for the earliest investigation as representative of a usual type of simple manual skill. The aim of a second study was to find the rate, and the manner in which simple sensory subject matter that has no meaning to the learner establishes and loses association strength. In the experiments discussed in the present chapter, the reaction to be acquired is highly complex. An effort was made to combine in it as many as possible of the conditions essential to the delineation of the curve of forgetting and to the comparison of the methods of securing data to that end.

In order to learn and to forget in a thoroughly typical way, I endeavored to choose one of the common tasks of learning in which the reaction was clearly sensori-motor, with the intellectual element present and dominant in the early stages of learning but subordinate later. This kind of material seemed desirable because investigations have shown that in order to exemplify the most usual procedure in learning, the early stages should demand much more than the mere sensory and motor reactions. The later stages should progress most rapidly when thought is most actively supplying devices to abbreviate the acts, and to rectify errors. At last, the presence of a stimulus should result in motor response more readily without the intervention of thought than when thought is present. As most learning passes through these stages, numerous examples were at hand; but typewriting was believed to fulfill the requirements best owing to the fact that there are necessarily not only pairs of associates consisting of members of a copy as stimuli to be related to members of a keyboard, but also a consecutive series, each member of which is a pair of associates because the letters are combined as words and sentences and the position of each key becomes known through its direction and distance from every other key as used in a series. Owing to these complexities the learning of typewriting requires much intelligent guidance. It is typical of the most practical accomplishments of the pupil in school, such as penmanship, read-

ing aloud, drawing, and the fundamental processes in numbers. The operation of a typewriter was therefore chosen as the subject matter of this group of experiments.

The typewriter was a Remington, No. 6, with the writing invisible. A screen was so placed as to conceal the keyboard from the reagent and thus to insure his confining himself to the so-called "touch method." On the screen at about reading distance from the reagent was placed a copy of the series to be learned. A paper in the machine preserved the evidences of the keys which had been struck. The subject sat in an office chair which could be adjusted to his most convenient height. A stop watch was used by the experimenter to measure the time required in writing the series. Whenever *B,* the writer, served as subject, it was necessary for him to perform the duties of experimenter also. The watch was then operated by means of the following device: A small wooden base was clamped near the edge of the top of the typewriter desk. Two metal brackets mounted on this base supported a seven inch wooden lever in such a way that it could be rotated about its middle point. Between one arm of the lever and the base, the watch was held on edge, with its stem in a small round depression in the lever, and the edge opposite the stem was fitted into an oblong depression in the base. A downward pull upon this arm of the lever started or stopped the second hand, or returned it to zero. This pull was made by the pressure of the foot upon a pedal. One end of this pedal was hinged to a foot stool, and the other end transmitted the downward motion to the arm of the lever by means of an ordinary bird-cage chain and spring. The spring was employed to lengthen the "life" of the stop-watch. When the pressure was released, the lever was restored to a horizontal position by the combined action of the spring in the stem of the watch and a small coil spring that drew the other arm of the lever towards the base.

Six reagents served in this group of experiments. All were graduate students in psychology except one. This one, *V,* was the only woman in the group. She was a junior in college, and had completed an introductory course in psychology. *B*'s purpose in serving as a reagent was primarily to gain first-hand critical knowledge of the methods. If *B*'s reactions had been perceptibly affected by knowledge of the experiment or by any anxiety when the method disclosed its weaknesses, his data would not have been included in the results. The absence of any indication of such influences signifies that the keeping of the observers in ignorance of their time records was a needless precaution. The writer, *B,* was one of the five who were beginning typewriting. *C* was the only subject who knew the

arrangement of the keys on the keyboard before beginning the experiments. He had begun the third stage on a similar keyboard, the Oliver, but was quite out of practise. The others were prompted at first by a diagram of the keyboard, which was placed on the screen below the copy. After having located the desired letter on the diagram, there remained the difficulty of striking its key. As it was usually quicker and easier to recall the motor action associated with a letter than to find it, the diagram was used only when necessary.

During the first stage, each series consisted of seven letters of one bank of keys used in mixed order three times. These twenty-one letters were presented in typewritten form, and were separated by spaces into groups of three letters each. This grouping of the letters made the series easier to read. When the reagents became able to locate the keys readily they practised until groups of letters began to become units of reaction and then were initiated into experiments in the second stage. During this second stage of practise the subject matter of the series was a phrase of five words. These phrases were so selected as to present as equal degrees of difficulty as possible. At the beginning of each experimental period a record was made of the reagent's condition for work, and of what he had been doing before the experiment. Then owing to the fact that the mind requires time to adjust itself to an occupation, a rather difficult series was written three times as a warming-up exercise. Following this the series that was practised during the previous period, was tested by three repetitions. Finally the subject practised a new series thirty times. The manner of repeating each of these series was as follows: When the experimenter gave the signal, "Ready," the reagent placed his hands in any position of preparedness on the keyboard that he chose. He refrained from lowering his eyes to the copy, until the experimenter started the watch and at the same time signaled, "Begin." The reagent wrote the series once, and immediately after striking the last key said "Now"; whereupon the experimenter stopped the watch. A rest of thirty seconds then intervened during which the experimenter recorded the time, his observations, and the introspections of the reagent. The rest after every tenth repetition was lengthened to one minute to give an opportunity for the recording of more detailed introspections. On the completion of thirty repetitions, the experimenter induced the retrospection of the process as a whole by means of questions. An interval was allowed to elapse before the series practised was tested and a new series learned. The lengths of these intervals were 4, 7, 14, 21, 28 and 35 days. One hundred and twenty-four experiments were performed in this group. However not all results could be included in

the tables owing to unusual conditions. Besides, reagent *E* left school before the experiments in Tables XVI. and XVII. were completed. As is usual in studies of forgetting, the curve is found in terms of the amount of gain that eventually has been lost through delay. The average time required in writing the series the first three times, minus the time consumed in writing it the last three times during practise, served as the measure of the gain. In like manner, the loss was found by subtracting the average of the three repetitions at the close of practise from the average of the test three given after the interval. Therefore the amount that was forgotten was equal to the loss divided by the gain.

TABLE XIII

First Stage in Typewriting

Seconds Gained through Practise and Lost through Forgetting

Reagents	*M*		*P*		*E*		*B*		*V*	
	Gains	Losses	Gains	Losses	Gains	Losses	Gains	Losses	Gains	Losses
1 day	27.3	3.9	26.3	3.1	18.3	.5	29.6	9.7	39.9	8.7
4 days ...	23.5	6.4	19.0	6.1	24.2	3.5	32.4	18.1	23.1	7.3
7 days ...	8.8	2.4	8.7	4.5	19.8	10.2	27.4	16.2	24.0	13.6
14 days ...	9.5	4.3	27.0	10.9	36.4	24.2	17.2	11.0	61.9	38.3
21 days ...	11.8	7.7	33.0	18.8	18.4	16.7	16.7	12.3	26.6	18.9
28 days ...	7.2	4.5	8.6	5.5	5.9	5.3	39.3	27.7	6.0	4.8
35 days ...	5.7	4.9			21.7	15.3	40.4	39.8		

TABLE XIV

First Stage in Typewriting

Per Cent. Forgotten

Reagents	*M*	*P*	*E*	*B*	*V*	Average
1 day	14.3	11.8	2.7	32.8	21.8	16.7
4 days	27.2	32.1	14.5	55.9	31.6	32.3
7 days	27.3	51.7	51.5	59.1	56.7	49.3
14 days	45.1	40.4	66.2	63.9	61.8	55.5
21 days	65.2	56.9	90.7	73.7	71.1	71.5
28 days	62.5	63.9	89.8	70.5	80.0	73.3
35 days	85.9		70.5	98.5		84.9

The results of practise on the typewriter previous to the stage in which words begin to act as wholes are the subject matter of Tables XIII., XIV., and XV. These tables show that the amount forgotten was, during the first day more than ⅕, by the end of 4 days nearly ⅓, during 7 days ½, and during the entire period, 35 days, more than ¾. However, there was little loss after the 21st day; for ½ of it took place within the first 5 days. Nearly ½ of the loss of the first week had taken place by the close of the first day. As observer

TABLE XV

C's Results

	Gain	Loss	Per Cent. Forgotten
1 day	13.0	2.3	17.7
4 days	16.6	4.1	25.3
7 days	13.1	3.8	29.0
14 days	86.3	38.7	44.8
21 days	12.0	6.3	52.5
28 days	11.5	5.8	50.4
35 days	24.3	13.6	55.9

C had reached the expert stage in typewriting at a previous time; but was out of practise when he began to serve in these experiments, and as the other observers were all beginners, his results were collated in Table XV. His losses were, during the first day less than 1/5, during the first 4 days 1/4, during the first 21 days 1/2, and during the 35 days, 3/5. It is evident that only 1/10 was forgotten during the last two weeks of the period that was studied. *C* wrote as slowly as the others during the early experiments and made as large gains but smaller losses. His general progress, therefore, was much more rapid than that of the beginners until he approached the fastest speed that he had ever attained. Both his gross gains and losses then became small; but the per cent. of his losses throughout the period averaged about 3/4 of that of the beginners. The results of the writer, *B*, were allowed to be a part of Tables XIII. and XIV. because they showed no advantages or disadvantages arising from his more intimate knowledge of the purpose and plan of the experiments and of his own time record and errors.

TABLE XVI

Second Stage in Typewriting

Seconds Gained through Practise and Lost through Forgetting

Reagents	*M*		*P*		*B*		*V*	
	Gains	Losses	Gains	Losses	Gains	Losses	Gains	Losses
7 days	19.8	7.9	27.8	5.8	14.9	5.6	16.5	3.8
14 days	9.1	6.2	19.1	11.2	17.2	7.6	7.7	5.9
21 days	11.8	9.9			14.1	10.3	26.6	18.9
28 days					19.6	18.7		

TABLE XVII

Second Stage in Typewriting

Reagents	Per Cent. Forgotten				
	M	*P*	*B*	*V*	Averages
7 days	39.9	20.9	37.2	23.6	30.4
14 days	68.1	58.6	44.2	76.6	61.9
21 days	83.9		73.0	71.1	76.0
28 days			94.5		94.5

TABLE XVIII

SECOND STAGE. *C*'S RESULTS

	Gains	Losses	Per Cent. Forgotten
7 days	19	— 4	—21.1
14 days	11.5	6	52.2
21 days	50.5	12	23.7
28 days	92	—20	—21.7

Tables XVI., XVII., and XVIII. are composed of the results obtained after it had become no longer necessary to think separately of each letter and its corresponding key; and when words began to serve as units in this period the progress in learning was slower than in the previous period, and the rate of forgetting was even slower. The amount lost was, during the first 7 days almost ⅓, during the first 14 days ⅔, and during the first 21 days more than ¾. Nearly all that had been learned was forgotten by the end of 28 days. As shown in Table XVIII., by the time the beginners were in this stage of typewriting, *C* was no longer making definite losses within intervals as short as those which we have been studying.

TABLE XIX

SECOND STAGE. FEW REPETITIONS IN PRACTISE

Intervals	Gross Gain	Gross Loss	Per Cent. Forgotten
5 minutes	141	25	18.4
20 minutes	140	48	35.3
60 minutes	141	77	54.6

In Table XIX. are presented the results of experiments like those in Tables XVI. to XVIII. except that the sentences were practised twenty times instead of thirty. *B* was the only subject. Four sentences were tested after five minutes and showed a loss of one fifth of what had been gained. Four were allowed an interval of twenty minutes with a loss of one third. Finally four sentences were tested sixty minutes after the end of practise, and resulted in the forgetting of two thirds of what had been learned. Therefore Table XIX. shows greater losses during the first 20 minutes and almost as great losses during 60 minutes as took place according to Table XVII. in the first 7 days and the first 14 days. It is clear then that if subject matter of this kind is well memorized the losses are much slower than when the association links are not so thoroughly established; but that the last part of the curve rises more slowly with twenty repetitions than with thirty repetitions.

The time of writing a series was often decreased at the expense of accuracy. On the other hand, in the effort to avoid all inaccuracies the subjects purposely diminished the speed in parts of the series

where mistakes could be avoided without especial care. In these cases the least suggestion of carefulness, such as was brought about by the discovery of an error in previous work, resulted in slower time on the next trial. In other words, when attention was focused on accuracy, the time was lengthened and rendered much more variable than when exactness was subordinated to speed. It seems expedient, for these reasons, for the reagents to exert themselves chiefly for rapidity, and at the same time, to endeavor to give accuracy as close a second place in consciousness as possible. In practise every error of order was accompanied, on an average, by 7 errors of omission and 9 errors of insertion. Practise reduced these omissions 49 per cent., the insertions 73 per cent., and the mistakes of order 50 per cent. during the first stage of practise. The average amount of this reduction of errors that reappeared owing to delays in recall was in the cases of omissions 26.3 per cent., of insertions 2 per cent., and of errors of order 88 per cent. It is evident that although the insertions were more numerous than the two other types of error, their number was reduced more rapidly by practise and was much more slowly increased by subsequent delay. Practise had almost equal effect upon omissions and cases of wrong order; but forgetting increased errors of order more than three times as fast as errors of omission. Hence, errors of order, although much less frequent than the other two types were the most difficult to get rid of by means of practise. This statement is also supported by the fact that *C*, who was in the third stage of typewriting, made relatively more mistakes of order than were made by other subjects.

The method which has been employed in the preceding experiments can be called a method of reproduction, as contrasted with the saving or relearning method of Ebbinghaus. As it has been employed in this chapter, the method of reproduction consists in first determining the gain in a certain associative performance due to a specified amount of practise, and then determining the loss of proficiency that results from a given interval of no practise. The loss divided by the preceding gain shows what proportion of the gain due to practise has been lost in a period of no practise, and therefore may properly be regarded as a measure of the amount forgotten. The results of this method are in general agreement with the familiar results of the saving method, but seem to be subject to more variation. It was now desired to institute a direct comparison between the two methods, with a view to ascertaining which gave the more regular and reliable results.

With this in view, an experiment was conducted in which both the method of reproduction, as above described, and the method of

relearning, were employed together. The performance studied was, as before, typewriting; and, as before, the experiment began with a specified amount of practise, which was followed by an interval of no practise. After this interval, still as before, a test was conducted to determine how much of the gain through practise had been lost in the interval; but now this test was followed by a new period of practise, continued until the former high point of practise was again reached. The time required to relearn the performance and regain the former proficiency could be compared with the time required to reach this proficiency in the first period of practise.

The subject matter of each experiment consisted of a couplet from Longfellow's "Evangeline." As great care as possible was exercised in the selection of couplets that were equally easy to write on the typewriter. The subjects were two normal school students. Both were men whose age and degree of maturity was about the same as that of college juniors. As previous experiments have not given evidence in favor of keeping the observer ignorant of his errors and his time, and as the group of experiments was necessarily too brief for much to be accomplished in the touch method of typewriting, the writing and the keys were allowed to be visible, and the observer started and stopped the watch and reported the time. Nevertheless, all this work was done under close supervision. Before practise and before the final test, the alphabet was written twice as a warming-up exercise. The couplet was practised thirty minutes.

TABLE XX

METHOD OF REPRODUCTION

Seven Days Interval

Experiment		First	Last	1st Test	Gain	Per Cent. Gain	Loss	Per Cent. Loss		
H	III.	3.08	2.8	4.67	1.00	32.5	2.59	259.00		
	IV.	3.00	1.75	3.00	1.25	41.7	1.25	100.00	*H*'s Av. .	140.0
	V.	3.92	2.00	3.17	1.92	38.40	1.17	60.9	*G*'s Av. .	63.4
G	I.	16.00	7.00	13.50	9.00	56.2	6.50	72.2	Total Av.	101.7
	V.	2.91	1.87	2.83	1.05	36.1	0.96	91.4	A.D.	51.6
	VI.	13.50	6.00	8.00	7.50	55.6	2.00	26.7	P.E.	17.3

An interval of one half minute followed each repetition as a rest, and as an opportunity to record the time and to report introspections. Each series was relearned to its former degree of proficiency either seven days or fourteen days after it had been first learned. The time required to write the couplet the first time and the last time in practise and also the first time in the relearning of it were used as the means of determining the gain and the loss, by the method of reproduction. These results are found in Tables XX. and XXI.

TABLE XXI

METHOD OF REPRODUCTION

Fourteen Days Interval

Experiment		First	Last	1st Test	Gain	Per Cent. Gain	Loss	Per Cent. Loss		
H	I.	20.00	8.00	10.08	12.00	60.0	2.08	17.3	Total Av.	31.4
G	IV.	4.00	2.00	2.91	2.00	50.0	0.91	45.5	A.D.	14.1
									P.E.	8.5

The number of repetitions in learning and in relearning were the data of the relearning method, and were used in constructing Tables XXII. and XXIII.

TABLE XXII

METHOD OF RELEARNING

Seven Days Interval

Expeiiment		Repetitions in Learning	Repetitions in Relearning	Per Cent. Necessary in Relearning		
H	III.	13	4	30.8		
	IV.	13	5	38.5	*H*'s Av. ..	39.8
	V.	4	2	50.0	*G*'s Av. ..	44.8
G	I.	4	2	50.0	Total Av.	42.3
	V.	9	4	44.4	A.D.	5.8
	VI.	5	2	40.0	P.E.	2.0

TABLE XXIII

METHOD OF RELEARNING

Fourteen Days Interval

Eqperiment		Repetitions in Learning	Repetitions in Relearning	Per Cent Necessary in Relearning		
H	I.	3	2	66.7	Av.	66.7
G	IV.	6	4	66.7	A.D.	0

The tables show that according to the method of relearning the losses of the two reagents are almost equal. The average deviations and probable errors in Tables XXII. and XXIII. are such as would indicate relative freedom from erroneous measures. The average losses of both reagents are according to the method of reproduction greater in 7 days than in 14 days. Besides, *H*'s average per cent. in the 7 days according to the table of reproduction is 2.3 times *G*'s average; but according to the table of relearning the two averages are almost equal. In the reproduction table for 14 days, *G*'s average per cent. is nearly three times that of *H*. But in the corresponding table of relearning the per cents lost are exactly equal. In Tables XX. and XXI. the average deviations are about half as large as their respective averages and the probable error in Table XX. is ¼ as great as its average. On the other hand in Table XXII. the average is more than seven times as large as the average deviation

and more than ten times as large as the probable error, and in Table XXIII. the average deviation is zero. There is no doubt, therefore, of the superiority of the results in the tables of measurements by relearning to the tables of measurements by reproduction. However, differences between reproduction and relearning are not much greater in these tables than might be expected, in view of the fact that only one measure, instead of the average of three measures, as elsewhere in the chapter, was the basis of the calculations of gain and of loss in reproduction. The method of reproduction is subject to large chance errors in the single measurement.

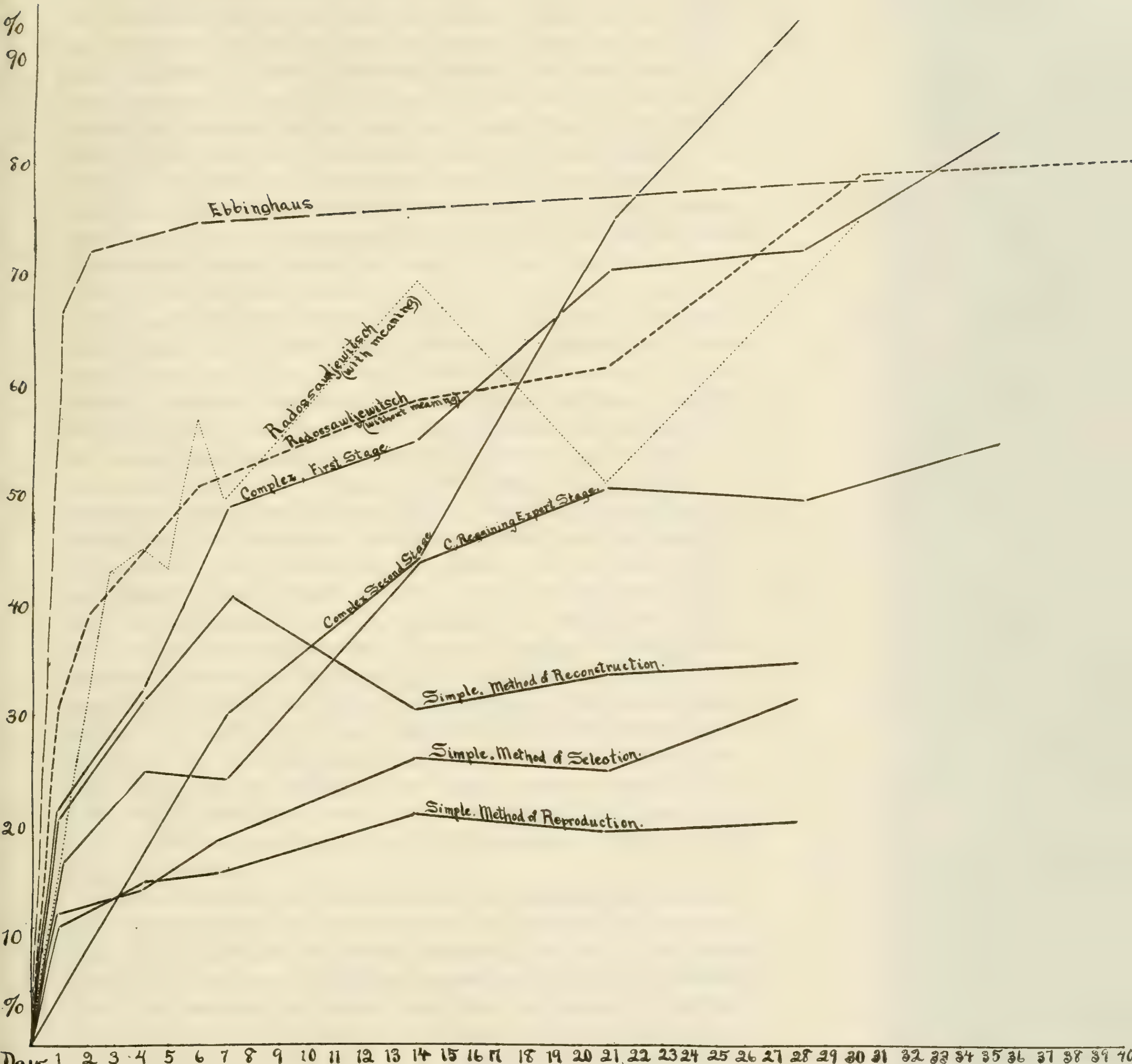
The results of this chapter may be summarized as follows: The curve of forgetting has been determined for the complex associative performance of typewriting a specified list of letters or of words. This curve has been found to have the same general character as that familiar from the work of Ebbinghaus on much simpler performances; the similarity consists in a more rapid rate of forgetting soon after cessation of the learning than later. But the actual rate of forgetting has been found to depend very largely on the amount of learning preceding the period of forgetting. When the amount of learning is small, the forgetting is at first rapid, but when the amount of learning is greater the forgetting starts at a much slower pace than is shown in the Ebbinghaus curve. In the latter case, the rate of forgetting is not excessively rapid at first, and, though it decreases, it decreases more slowly than in the usual curve.

CHAPTER VI

Discussion of Results and General Conclusions

It will be remembered that this investigation is (1) a further study of the curve of forgetting as to its form and as to the rate of loss in association strength after practise has ceased, and (2) a search for reasons for the dissimilarities in the curves already found.

The curves that have been obtained by others in the more extensive investigations of forgetting are shown in dotted lines in Fig. 3. The curve of Ebbinghaus for nonsense syllables rises with great rapidity during the first day, undergoes a marked decline in rate during the following five days, and ascends slowly during the remaining 25 days. Radossawljewitsch's curve for the same kind of material makes its rapid ascent within the first two days. The change to slower rates of loss is somewhat more gradual than that of Ebbinghaus; but the latter portion of the curve shows a more rapid rate of forgetting than was found by Ebbinghaus. My curves for meaningless subject matter indicate that association strength decreases remarkably during the first day. Almost all of the loss that is shown by the curve which was determined by the method of selection had taken place by the end of the first week. The curve found by the method of reproduction rises very little after the second week. Radossawljewitsch's curve for the forgetting of matter with meaning does not differ greatly from his curve for meaningless subject matter except that it is much more irregular and therefore more difficult to interpret. His curve for material with meaning is slower than his other curve until the end of the third day, and indeed it indicates less decline in the ability to learn after an interval of a month. My curves for typewriting show a great loss of association strength during the first week and a gradual falling off in the amounts forgotten during the remainder of the month. This diminution in the amount forgotten is not so evident in the curve of the second stage as in that of the first stage of typewriting. Observer *C*, who had once attained considerable ability in typewriting but was out of practise when he began to serve as subject, produced a curve that rises rather rapidly during the first day, at a gradually decreasing rate to twenty-one days, and slowly thereafter. In his second stage his curve, if it were delineated, would be so irregular that it would give no evidence either of gain or of loss. It is evi-



%
90
80
70
60
50
40
30
20
10
%
Ebbinghaus
Radossawljewitsch (with meaning)
Radossawljewitsch (without meaning)
Complex, First Stage.
Complex Second Stage
C. Regaining Expert Stage.
Simple. Method of Reconstruction.
Simple. Method of Selection.
Simple. Method of Reproduction.
Days 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39 40

FIG. 3.

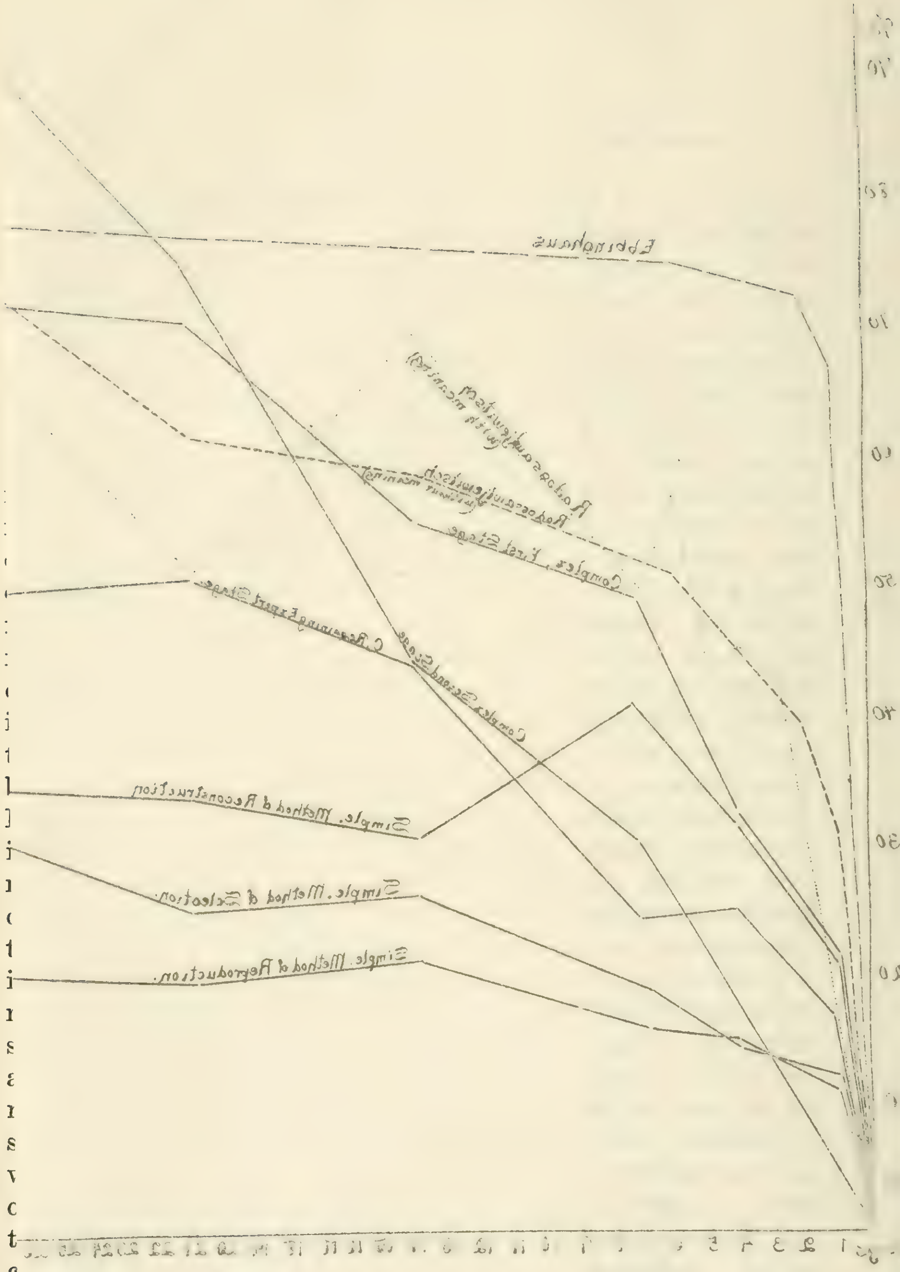

Ebbinghaus
Radossawljewitsch (with meaning)
Radossawljewitsch (without meaning)
Complex, First Stage
C. Reasoning Expert Stage
Complex Second Stage
Simple. Method of Reconstruction
Simple. Method of Selection
Simple. Method of Reproduction
70
60
50
40
30
20
10

Fig. 3.

dent that to a considerable extent the form of the curve of forgetting resembles that of the curve of learning. However, the forgetting curve for nonsense syllables has a more abrupt change from its rapid rise than is found in memory curves. This short turn in the curve is probably due to the primary dependence of the memory of meaningless subject matter upon the innate retentiveness of the brain centers that are immediately involved; and, owing to the poverty of meaning, it is dependent only in a secondary way upon the few indefinite interactions that can be set up with other centers. These are such trivial relations that they rapidly decline, and after a remarkably short period, the slowly fading impressions of the other sort are all that remain.

The curve of forgetting may be studied (1) as to its general form; and (2) as to the absolute rate of forgetting. Suppose the general form of the curve to be constant, and to have the roughly ''logarithmic'' character signalized by Ebbinghaus—a form indicating that the rate of forgetting is most rapid at first and becomes slower and slower—and suppose further that the absolute rate of forgetting varies with conditions. Then it is clear that a rapid absolute rate of forgetting would make the rise of the curve very steep at first, and would lead to an apparently abrupt transition from this rapid forgetting to the slower forgetting that ensues. On the other hand, a slow absolute rate of forgetting would cause the initial rise of the curve to be less steep and the transition from this to the ensuing portion of the curve less abrupt. But such changes leave the general character of the curve unchanged; they affect only a certain ''parameter'' of the curve, and could be obliterated, in the graphic representation of the facts, by simply altering the horizontal scale of the graph, making this scale proportional to the absolute rate of forgetting. Reducing the horizontal scale would change a slowly rising curve, with a gradual transition from the initial to the later phase, into a rapidly rising curve with an abrupt transition from the initial to the later phase.

Inspection of the various curves in Fig. 3 shows that the differences between them are approximately of the sort just described. They differ in the absolute rate of forgetting, but not in the general character of the curve. All belong approximately to the logarithmic type. The results of the present study have not brought to light any curve of forgetting of a definitely different type. Such divergences as have appeared are probably to be considered as accidental variations. Much the same can be said of the divergences from the logarithmic type of curve which are emphasized by Radossawljewitsch[9]

[9] *Op. cit.*

in particular the apparent recovery between 8 and 24 hours. He found that less was forgotten in 24 hours than in 8 hours—or, at least, that the saving in relearning was greater after 24 than after 8 hours. This may reasonably be attributed to the fact that the 8-hour interval brought the relearning into an unfavorable time of the day, while the 24-hour interval allowed sleep to intervene and brought the time of relearning back to the original hour of the day, with all conditions, external and internal, most nearly identical with those of the first learning.

In brief, a survey of previous results as well as of the results of the present study gives no warrant for altering the general character of the Ebbinghaus curve of forgetting, nor for introducing any other general type of curve alongside of that.

But as regards the absolute rate of forgetting, the literature of the question, as well as the experiments herein reported, gives abundant evidence that this is a variable quantity. The rate of forgetting depends on the following factors:

1. It depends on the degree to which the material has been learned before the commencement of the period of forgetting. This was already indicated in the original experiments of Ebbinghaus, since he found that any study of the material beyond the point necessary for one correct reproduction made it easier to relearn the material later, or, in other words, diminish the degree of forgetting after a given interval. The rate of forgetting was slower when "overlearning" had occurred than when the study had been barely sufficient to reach the standard of one correct recitation. It is probable that the slow rate of forgetting found by Radossawljewitsch was the result of his requiring such an amount of study as would make possible *two* correct recitations in succession, instead of only one; for this author found that the amount of study needed for two correct recitations was often considerably more than that required for one correct recitation. It is likely that others of the discrepant results in the literature, in regard to the rate of forgetting, depend on this matter of overlearning.

In one of the experiments of Chapter V., a direct comparison is instituted between the rates of forgetting when preceded by different amounts of learning. When the subject of this experiment practised a list of words on the typewriter 30 times, he forgot, after 7 days 37.2 per cent., after 14 days 44.2 per cent., after 21 days 73.0 per cent., after 28 days 94.5 per cent. But when the initial learning was limited to 20 repetitions, he forgot 18.4 per cent. in 5 minutes, 35.3 per cent. in 20 minutes, and 54.6 per cent. in 60 minutes. The curves of forgetting in the two cases have the same general form,

but the absolute rate of forgetting is much more rapid after the smaller amount of practise.

A method developed in this same chapter, though not employed specifically for the problem now under discussion, seems specially adapted to its study, in that it permits of a measure of the amount of "overlearning" in terms of proficiency acquired instead of simply in terms of the number of repetitions. The proficiency being measured by the speed of performance, the subject practises a given performance till he reaches a certain degree of proficiency, and then, after an interval of forgetting, practises again, or relearns, till he reaches the same degree of proficiency as before. In different experiments, the degree of proficiency to be attained can be varied, and the curve of forgetting thus traced from different starting points. Though the test has not been made with exactly this method, the probability is, from the results at hand by a slightly different method, that the curves of forgetting would have the same general form from whatever level of proficiency they start, but that the rate of forgetting would become slower and slower as the initial level of proficiency was raised. Book,[10] it will be recalled, after attaining a considerable degree of proficiency in the use of the typewriter, interrupted practise for a year, and then found but a few minutes fresh practise needed to reach his former level. The saving method of computation here indicates a forgetting of less than 1 per cent. in a year.

2. The rate of forgetting probably depends on the distribution or concentration of the process of learning. This is indicated by results of Ebbinghaus, Jost and others. When the practise leading to a certain degree of proficiency has been distributed over several days, the rate of forgetting is slower than when the practise has been concentrated in time, though the proficiency reached be the same in the two cases.

3. The rate of forgetting varies with the performance or material learned. It is slower for meaningful than for nonsense material. Aside from meaning in the intellectual sense, it is probable that other differences in the kind of performance affect the rate of forgetting. Thus skill in throwing at a target (Chapter III. above), though slowly acquired, was slowly forgotten—almost too slowly to furnish the basis for a study of the curve of forgetting. Again, in the typewriting experiments (Chapter V.), though the number of repetitions constituting the first learning of a performance was not greater than is often necessary in learning a list of nonsense syl-

[10] "The Psychology of Skill," p. 76.

lables, yet the rate of forgetting of the typewriting act was noticeably slow. Such performances as typewriting or throwing at a target differ from the recitation of lists of nonsense syllables in possessing a sort of motor meaning or significance. They do not appear, introspectively, as meaningless performances, but seem to accomplish something, and it is possible that this practical meaning is like meaning of the more intellectual sort in favoring the retention of what has been learned.

4. The rate of forgetting varies according to the method by which it is measured. Different rates are to be expected according as retention is measured by power of reproduction or by time of relearning. The results in Chapter V. would indicate, though not very reliably, that the rate of forgetting is more rapid when measured in terms of reproduction than in terms of relearning. Reproduction seems certainly the more direct test of the present strength of old associations, and relearning measures, rather the effect of fresh study upon the old associations; but the method of reproduction is the more subject to accidental error.

The method of reproduction gives different results according as time or accuracy is the criterion; and if accuracy is the criterion, the rate of forgetting comes out differently according to the kind of errors that are counted. If omissions are counted, forgetting appears to occur more rapidly than when insertions are counted. Insertions, the easiest errors to eliminate through practise, are the slowest to reappear during the intermission of practise. Errors of order are rather difficult to eliminate, and quickly reappear after intermission of practise. It is clear that these divergences of result, according to the unit of measurement, are not simply indications of unreliability in the methods, but are evidences that any statement of the rate of forgetting, without specification of the kind of loss, have little if any meaning. Measurements in terms of time have usually been treated as if free from such limitations; but this is not justified unless there is constancy in all these other respects.

Further, the rate of forgetting comes out differently according to the manner of recall adopted in the test for power of reproduction (Chapter IV.). This is by reason of the fact that one form of recall makes use of one kind of elements of the past experience, and another form of recall another kind of elements. When the method of reconstruction is employed, the subject can neglect all phases of the past experience except the original order of the members of the series. The method of selection or recognition tests only the ability to recognize the former experiences when they are again presented. The reproduction of the whole series in proper order would seem to

make the greatest demands upon the associations and to be the most complete method. But the curve of forgetting can properly be measured by any of these methods, and the curve, or at least the rate of forgetting, differs with the method. Order is forgotten with great rapidity at first and much less rapidly later. The ability to recognize is lost rather gradually. The ability to reproduce is lost rather rapidly at first, and the transition to the period of slow forgetting is sometimes gradual and sometimes abrupt.

5. The rate of forgetting shows individual differences in retentiveness. The present study has not revealed any sex difference in this respect. It appears, from the work of earlier investigators, that the rate of forgetting depends on age. Probably, the other factors concerned in forgetting being equal, young children forget more rapidly than adults.

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The readers of this paper whose teachers have manifested an interest, not only in the solution of the problems of their studies, but also in their problems of life, can most fully appreciate my gratitude towards those who have instructed me by means of this research, and have given me encouragement when I needed it most. Professor James R. Angell first launched me in this investigation, and Professor John Broadus Watson rendered helpful advice. Professors J. McKeen Cattell, E. L. Thorndike, and R. S. Woodworth generously spent many hours with me in interviews that were replete with valuable suggestions. I wish to acknowledge also my obligations to the large number of persons who served as reagents.

THE INFLUENCE OF CAFFEIN ON MENTAL AND MOTOR EFFICIENCY

Y

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CONTENTS

THE INFLUENCE OF CAFFEIN ON MENTAL AND MOTOR EFFICIENCY

CHAPTER I

Summary of Previous Investigations

The literature on the influence of caffein on fatigue has been so recently summarized and the unreliability of the older experiments so clearly pointed out by Rivers[1] that it seems superfluous to go over the field again in any great detail. Hence the present chapter will give only a brief summary of the most important researches which have had as their object the determination of the influence of caffein on mental and motor processes. No attempt will be made to discuss the physiological effects of the drug, since such summaries can be found in any of the standard works on pharmacology.

In 1892 increase in the force of muscular contractions was demonstrated by De Sarlo and Bernardini[2] for caffein and by Kraepelin for tea. These investigators used the dynamometer as a measure of the force of contraction. Most of the subsequent work on motor processes has been by the ergographic method. Ugolino Mosso,[3] Koch,[4] Rossi,[5] Sobieranski,[6] Hoch and Kraepelin,[7] Destree,[8] Benedicenti,[9] Schumberg,[10] Hellsten,[11] and Joteyko[12] have all demonstrated the stimulating effect of caffein on ergographic performance, the drug being administered in such various forms as caffein, tea, coffee, kola, mate, guarana and theobromine. Only one investigation of those reported by Rivers failed to find an appreciable effect, that

[1] W. H. R. Rivers, "The Influence of Alcohol and Other Drugs on Fatigue."

[2] *Revista sper. di Freniatria*, **18**, 1.

[3] *Archiv ital. de Biol.*, **19**, 241, 1893.

[4] Inaug. Diss., Marburg, 1894.

[5] *Revista sper. di Freniatria*, **20**, 458, 1894.

[6] *Centralbl. f. Physiol.*, **10**, 126, 1896.

[7] *Psychol. Arbeit.*, **1**, 378, 1896.

[8] *Journal Med. de Bruxelles*, 1897.

[9] *Moleschott's Untersuchungen*, **16**, 170, 1899.

[10] *Archiv f. Anat. u. Physiol.* (Physiol. Abth.), Suppl. Bd., 1899, 289.

[11] *Skand. Arch. f. Physiol.*, **16**, 197, 1904.

[12] *Travaux du Lab. de Physiol. Inst. Solvay.*, **6**, 361, 1904.

of Oseretzkowsky and Kraepelin.[13] Feré[14] affirms that the effect is only an acceleration of fatigue.

Although there is general agreement here as to the presence of stimulation there is some dispute as to whether only the height of the contractions or their number or both are affected. The quantitative results have also varied considerably, as might be expected under such great diversity of methods as has been followed. Carefully controlled experiments by Rivers and Webber[15] "confirm in general the conclusion reached by all previous workers that caffein stimulates the capacity for muscular work; and it is clear that this increase is not due to the various psychical factors of interest, sensory stimulation and suggestion which the experiments were especially designed to exclude. The greatest increase . . . falls, however, far short of that described by some previous workers such as Mosso, and it is probable that part of the effect described by these workers was due to the factors in question." Recent experiments by H. C. Wood, Jr.,[16] on the influence of caffein on isolated frog muscle bear in the same direction.

Investigations of mental processes under the influence of caffein have been much less frequent. Dietl and Vintschgau[17] found a striking and prolonged acceleration in the simple reaction time 20–25 minutes after drinking two cups of black coffee. Dehio[18] concluded, on the basis of inadequately controlled experiments, that coffee and tea did not exert any considerable effect on simple reaction times, produced a very slight acceleration of choice reaction times, but stimulated to a striking degree word reactions and association reactions. The effect began in about 15 minutes after the drink and lasted longer in the case of the association reactions than in any other case (over 1 hr.). The acceleration is said to have been sometimes followed by a retardation, but the different tests did not agree on this point and it is suggested that the retardation is only a normal fatigue phenomenon. These results are, however, quite unreliable, for the tests were often made on the evening of days on which ether, chloroform or alcohol had been taken in the morning, neither control doses nor control subjects were used, and no normal records were taken on the subjects tested. The tests, moreover, lasted for only a

[13] *Psychol. Arbeit.*, **3**, 617, 1901.

[14] *C. R. de la Soc. de Biol. Paris*, 593–627, 1901.

[15] W. H. R. Rivers, *op. cit.*, p. 38.

[16] Paper before Section on Medicine, College of Physicians of Philadelphia.

[17] "Das Verhalten der physiologischen Reactionszeit unter dem Einfluss von Morphium, Kaffee und Wein," *Pflügers Archiv*, **16**, 316, 1877.

[18] "Untersuchungen über den Einfluss des Caffeins und Thees auf die Dauer einfacher psychologischer Vorgänge," Diss., Dorpat, 1887.

short time, the variations found were quite within the normal range of variability for such limited number of observations as were made; no attempt was made to eliminate practise or fatigue; and no allowance was made for sensory stimulation, excitement or interest.

Kraepelin's[19] earlier attempts to retest Dehio's results on association times are equally useless, since the results are affected by a practise fall of 50 per cent., the tests were performed at different times of day, on days irregularly interspersed among alcohol experiments, and the dose was not caffein in any of its pure forms, but a brew of tea, with no control doses. Kraepelin's attempt to determine the influence of tea on the *content* of the associations is similarly meaningless. In still later experiments, although by defective methods, Kraepelin found acceleration in addition, retardation in speed of memorizing digits, increase in speed of reading, but is not positive that the changes pointed out were really due to the influence of the tea. He concludes that tea facilitates the perception of external impressions (Auffassung ausserer Eindrücke) and the combination of ideas (Verbindung der Vorstellungen). The latter factor is less influenced in the case of much practised and stereotyped associations. The increase in work Kraepelin believes to be due entirely to these two changes, and supposes that no acceleration of simple motor processes (Auslösung der Muskelbewegung) takes place. The increased force of contraction in dynamometric tests, which persists for a shorter time than the above influences, is explained as due to the direct peripheral action of the caffein content on the irritability of muscle tissue.[20]

Kraepelin concludes, finally (p. 224):

"The picture of the tea influence which we have thus secured agrees in all essential points with the experiences of daily life. We know that tea and coffee increase our mental efficiency in a definite way, and we use these as a means of overcoming mental fatigue. . . . In the morning these drinks remove the last traces of sleepiness and in the evening when we still have intellectual tasks to dispose of they aid in keeping us awake. In large amounts and in the case of sensitive persons their ingestion delays sleep. On the other hand, the motor disturbance, the tendency to boisterous and silly conduct, the peculiar euphoria and the exaggerated feeling of strength, characteristic of alcoholic intoxication, never result from the use of tea and coffee. In their places ensues a greater briskness and clearness of thought. . . . But secondary fatigue is either entirely absent or is very slight, whereas in the case of alcohol it follows quickly and inevitably."

[19] E. Kraepelin, "Ueber die Beeinflüssung einfacher psychischer Vorgänge durch einige Arzneimittel," pp. 107–148, 216–225.

[20] See Kobert, "Uber den Einfluss verschiedener pharmakologischer Agentien auf die Muskelsubstanz," *Archiv f. experimentelle Pathologie u. Pharmakologie,* **15**, 63.

In a still later piece of work Kraepelin and Hoch[21] investigated the influence, on performance in the addition test and in the production of ergograms, of the two chief constituents of tea—the caffein and the essential oils. Both the caffein and the oils were found to exert a favorable influence, which was greater in the case of the caffein. Paraguay tea had similar effects, which are attributed to its caffein content.

Ach[22] tested choice reactions and the speed and accuracy of the perception of words and nonsense syllables, in the case of two subjects, after .2 and .5 grams of caffein (whether alkaloid or citrated is not stated) administered in gelatine capsule. Control doses of common cooking salt were used on two days and caffein on three days. Control tests were also made before the doses on the caffein days. The results showed caffein to affect chiefly the accuracy and the time of perception, the influence showing itself in the form of improvement in both speed and number of mistakes. The choice reaction times show no clear effect, and the results, as a whole, are complicated by practise effect and by a considerable separation of two of the days from the other three. The caffein effect is said to begin about 20 minutes after the dose and to be measurably present after more than one and a half hours. There were indications that the caffein influence was stronger under conditions of fatigue.

Rivers[23] seems to have been the first to appreciate fully the genuine and practical importance of thoroughly controlling the psychological factors that are likely to play a rôle in such experiments. His analysis of these factors is pointed out in detail in the following chapter. Attempting to eliminate, by properly controlled procedure, all such factors as knowledge of the character of the dose, sensory stimulation, suggestion, excitement, interest and practise, Rivers investigated both motor and mental efficiency, with special reference to the course of fatigue. His results on force of movement, secured by the ergographic method, have already been quoted. The experiments involving more distinctly mental factors (chiefly perception and coordination) were on typewriting and on McDougall's "accuracy of aim" test. Caffein (.3 gram citrated), was found to increase the speed of performance in typewriting, but to have no influence on the accuracy. In the "accuracy of aim" test the caffein days were found to be superior to the control days. The chief objections to Rivers' experiments are the limited number of subjects (only him-

[21] *Psychol. Arbeit.*, **1**, 431, 1896.

[22] N. Ach, "Uber die Beinflüssung der Auffassunsfähigkeit durch einige Arzneimittel," *Psychol. Arbeit.*, 203–289, 1901.

[23] Rivers, *op. cit.*

self and a fellow worker), the insufficient length of time over which the effect of the dose was traced, and the small number of tests employed. His work is especially valuable for its emphasis on experimental technique in administering the doses.

Rivers' general conclusion is as follows:

> ''The general practical conclusion to be drawn from the experiments which I have recorded, and from those of previous workers, is that caffein increases the capacity for both muscular and mental work, this stimulating action persisting for a considerable time after the substance has been taken without there being any evidence, with moderate doses, of reaction leading to diminished capacity for work, the substance thus really diminishing and not merely obscuring the effects of fatigue. The results of one experiment, however, point unmistakably to the conclusion that, when taken in excess, the stimulating action may be so transitory and followed by so great a decrease that it may legitimately be spoken of as an accelerator of fatigue. . . . The experiment suggests strongly that caffein is a dangerous remedy as a stimulant in cases of prolonged fatigue, or of that enhanced tendency to fatigue which is the characteristic feature of neurasthenia'' (p. 50).

Langfeld,[24] in an investigation of the influence of caffein on ''suppression with negative instruction,'' has recently reported that ''caffein caused a decrease in the reaction time for association, and showed no appreciable effect upon the suppression or accuracy of reproduction.''

[24] *Psychological Review,* **18**, No. 6, 424, November, 1911.

CHAPTER II

The Purpose and Method of the Experiment

Previous experiments on the influence of caffein on psychological processes have been limited to a few tests of a very limited number of individuals, and have not been carried out under sufficiently rigorous experimental conditions. The processes tested in a given experiment have been few in number, the subjects have not been on a practise level of performance, the size of the dose has not been varied over any considerable range, nor has the drug been administered unmixed with other substances which have in themselves a measurable influence on the processes tested, nor has the caffein influence been traced for any considerable length of time. Hence in making the present elaborate series of experiments the following five chief purposes were held in mind. Of these five topics the first four can be appropriately discussed in the present monograph. The fifth must be left for a future series of papers.

Purposes

1. To determine both qualitatively and quantitatively the effect of caffein on a wide range of mental and motor processes, by studying the performance of a considerable number of individuals for a long period of time, under controlled conditions.
2. To study the way in which this influence is modified by such factors as the age, sex, weight, idiosyncracy and previous caffein habits of the subjects, and the degree to which it depends on the amount of the dose and the time and conditions of its administration.
3. To investigate the influence of caffein on the general health, on the quality and amount of sleep, and on the food habits of the individuals tested.
4. To inquire into the value and adaptability of a considerable array of simple tests with a view to their standardization for the purposes of pharmaco-dynamic research.
5. To accumulate data on the effects of practise, fatigue, diurnal variations in efficiency, the physiological limit, individual and sex differences and various other allied topics growing out of such an extended series of tests on a large number of subjects.

Plan and Procedure

In order to reduce to a minimum distractions and disturbances to which such experiments are likely to be subjected, and to provide for the greatest convenience of the experimenters and the comfort of the subjects, a well-lighted and ventilated six-room apartment on the ground floor of a building in a quiet part of the city was rented and equipped as a special laboratory. An abundance of chairs, tables, recording materials and files was secured, and special lights, batteries and the requisite psychological apparatus installed, including two motor test boards, two Columbia chronoscopes, ten stop watches, a stethoscope, illusion weights (3 sets), test blanks and the special tests to be described later. The tests were assigned to the various rooms and each room placed in charge of a competent assistant (see page 15).

Sixteen subjects, ten men and six women, were engaged for full time for a period of 40 days, and were required to appear at the laboratory at stated times during the day or to remain there permanently as the case might be, and to submit themselves at regular intervals to the series of mental and motor tests. These subjects were to abstain from the use of all forms of caffein (coffee, tea, chocolate, cocoa), alcohol, nicotine and all other drugs, as well as from soda fountain drinks containing patent syrups, except in so far as these drugs were prescribed by the director or by the medical assistant. They were also to observe regular hours of eating and sleeping and to report any unavoidable irregularities in these matters. Before beginning the experiment each subject subscribed to the following agreement:

It is Hereby Agreed

1. That I will abstain from the use in any form or quantity, of tea, coffee, tobacco, chocolate and cocoa, soda-fountain drinks containing patent syrups, alcoholic drinks (including beer) and all other such drugs, except at the prescription of the Director, so long as I shall serve as subject in this experiment.
2. That I will observe regular hours of eating and sleeping during the same period, taking my meals at the hours to be later prescribed by the Director.
3. That I will exert myself in every trial of every test to make the best and speediest record possible for me.
4. That I will conscientiously report to the medical adviser the condition of my health from day to day, along with any other items concerning which information may be desired.
5. That I will serve as subject in this experiment for as many of forty days as my assistance may be desired.
6. That I will appear regularly and promptly at the test hours.
7. That I will observe all other reasonable instructions which the Director may from time to time suggest for the good of the experiment.
8. That the failure to observe these instructions and conditions to the satis-

faction of the Director shall be deemed sufficient reason for forfeiting all compensation for any service I may have rendered up to that time and for terminating my connection with the experiment.

9. That I will if desired, at the close of the experiment, take oath before a notary public that I have lived up to the conditions of this agreement.

Signed,

The experiment as performed consists of three separate sections.

A. A series of tests covering a period of four weeks, in which all the subjects went through the tests five times a day (each time requiring about one hour) at 7:45 and 10:00 A.M. and at 12:15, 3:10 and 5:30 P.M. This arrangement left about one hour between tests, during which time the subjects were free to go about their own work or to remain in the laboratory reading, sewing, etc. During the first week, in order to allow all subjects to get perfectly adapted to the experiment and to become practised in the tests, sugar of milk doses were given to all individuals daily. After this week the caffein doses began. The subjects were then divided into four squads. To one squad (I.) consisting of four subjects, no caffein was administered throughout the experiment, but the control capsule, containing sugar of milk, was given daily. A second squad (II.) consisting of three subjects, alternated throughout the experiment, taking caffein on three days and sugar of milk on the following three days, at 10:30 A.M. This squad lunched between one and two o'clock. The caffein dose varied from one to six grains, the same amount being taken on each of a given set of three caffein days. A third squad (III.) consisting of three subjects, took caffein and sugar of milk on alternate days, at the lunch hour, the dose varying from one to six grains. Squad IV. consisting of five subjects took caffein and sugar of milk on alternate days from two and a half to three hours after lunch, which was, in these cases at 11:30 A.M. (For details concerning the character of the doses and the methods of administering them, see a later section of this chapter, p. 10.)

B. The second section, an intensive experiment of three days, was performed in order to study at close range the effect of caffein on the various processes tested, and to determine its time relations in the various cases and with the different subjects and squads. In this experiment the plan was followed of having all the subjects assemble at the laboratory at 10:00 A.M. At this hour the tests were begun and were kept up continuously for about 12 hours, except for two 45-minute periods allowed for lunch and dinner. All subjects ate at the same place at lunch and dinner of these three days, in order to eliminate variations due to differences in diet, condition of stomach, and to exercise going to and from the laboratory. The

meals were prepared and eaten in the house in which the laboratory was situated. By this means the action of the doses could be traced at close range, since all subjects returned repeatedly to the same test after having just passed through all the others in immediate succession. Fifteen records for each subject were thus secured for each test on each of the three days.

The same subjects were used as in the first section of the experiment, except that one man and one woman were absent throughout. These subjects were, then, all trained and practically on their practise level in the tests, having perfected themselves by the 140 trials during the preceding four weeks' experiment. In this section the subjects were again divided into four squads. Squad I. took caffein along with soda fountain syrup and carbonated water, after the 6th trial on the 1st and 3d days, and a sugar of milk capsule at the same time on the 2d day. Squad II. served as a control squad. On the first two days they took only sugar of milk capsules, but on the last day, after the 6th trial, they took 3 gr. of caffein, which was followed by 2 gr. more about an hour and a half after the evening meal. Squad III. took 3 gr. of caffein on the 1st day, after the 6th trial. On the two remaining days they took only sugar capsules, the object being to trace the action of the original dose for at least three days after taking. Squad IV. took 6 gr. of caffein on the first day, and only sugar capsules on the days following, the object being here, as in Squad III., to trace the action of the first dose during the following two days.

C. The third section of the experiment was designed to determine the effect of caffein when taken along with food substance in the form of syrup such as is commonly contained in soda fountain drinks, and to compare this effect with the action of the syrup when taken with no caffein ingredient. This experiment occupied 7 days. On two of these days no dose was administered to the subjects. These are called blank days, and they show what sort of performance one may expect in the tests employed when thoroughly practised subjects are used and no drug of any kind taken. On two other days all the subjects took doses of plain syrup, served with carbonated water so as to enable the effects of simple sensory stimulation to become apparent. On the remaining three days the same syrup was served in the same way, except that 1.2 grains of caffein alkaloid were added to each glass of the drink. On one day 1 glass of the caffeinated syrup was given to all subjects 15 minutes before beginning the tests at the 3:10 period. On another day 3 glasses and on the third day 5 glasses were given, the three amounts thus containing 1.2, 3.6 and 6.0 grains of caffein.

The tests during this third section of the experiment were held five times a day, as in section 1, and at the same time of day. When the one glass dose was taken it was served just before the 3:10 test. In the other cases a dose was taken before each of the afternoon tests. As subjects in this section, twelve of the persons used in the tests of the previous month were used. They were hence practised in all the tests, thoroughly familiar with the method of procedure and perfectly adjusted to the experimental conditions. The whole group of 12 acted as a single squad, so that all received the same dose at the same time of day. The subjects in no case knew whether they were taking the plain or the caffeinated syrup. In fact they knew nothing about the character of the dose except that the experiment was to determine the effects of soda fountain syrups on mental and motor processes.

Subjects

In going through the tests at the appointed hour the subjects came to the various rooms in squads of three, the identity of the squads being permanent throughout a given section of the experiment. The tests were so distributed in the various rooms and among the various assistants that all the five squads would complete their tests in the respective rooms at approximately the same time. At a whistle signal on the part of the director the squads shifted from room to room, and so on through the five shifts which completed the hour's work. Each subject was given a number by which he or she was known throughout the experiment, and by which reference to individuals will be made in the chapters to follow. The following table gives the number and name of all the subjects, along with their age, sex, weight, occupation, previous caffein habits, and the squad to which each belonged in Section 1 and Section 2 of the experiment. The writer wishes at this point to acknowledge his obligation to these subjects for their faithfulness and zeal throughout the experiment.

Doses and Their Administration

Many sources of error exist, in such experiments, in the character of the dose and in the way in which it is administered. The principal dangers have been so clearly pointed out by Rivers that I can do no better than quote, at this point, the following important paragraphs from his chapter on the action of drugs.

"I can now pass to a feature of method . . . designed to eliminate the influence of certain psychical factors which have undoubtedly been allowed to affect the results of nearly all who have experimented on the action of drugs. Many

TABLE I

THE SUBJECTS

No.	Name	Age	Weight	M—Male F—Female	Occupation	Caffein Habits	Squad Sec. 1	Squad Sec. 2
1	C. R. A.	39	?	M	Teacher	Regular	I.	II.
2	E. A. B.	38	?	F	Wife of College Instructor	Regular	Worked alone at typewriting	
3	A. E. C.	39	159	F	Wife of Graduate Student	Abstainer	III.	IV.
4	H. W. E.	19	124	M	Student	Moderate	I.	I.
5	R. N. G.	33	105	F	Wife of Teacher	Regular	IV.	—
6	W. A. J.	33	125	F	Wife of College Instructor	Regular	IV.	III.
7	S. A. F.	19	153	M	Student	Moderate	I.	II.
8	C. L. L.	24	144	M	Graduate Student	Regular	II.	III.
9	A. M. McC.	21	130	M	Student	Abstainer	III.	III.
10	C. H. N.	28	157	M	Law Student	Occasional	IV.	IV.
11	F. C. R.	27	110	F	Wife of Graduate Student	Abstainer	IV.	I.
12	K. E. R.	24	160	M	Graduate Student	Regular	II.	III.
13	V. H. R.	22	175	M	Student	Regular	II.	—
14	B. E. S.	27	193	M	Graduate Student	Occasional	III.	IV.
15	S. R. S.	34	108	F	Wife of College Instructor	Occasional	I.	II.
16	T. W. V.	24	174	M	Law Student	Regular	IV.	III.

of these workers have considered the possibility that their results may have been influenced by suggestion, or of bias towards results which were to be expected theoretically, and some have shown that effects similar to those following the administration of a drug may be the consequence of the administration of a wholly inactive substance which is supposed by the subject to be the drug in question. Few, however, have adopted the obvious precautions which such considerations suggest (that of using a control substance).

"The factor which previous writers have considered under the title of 'suggestion' is far from being the only source of error in work on the action of drugs. Feré has shown that the sensory stimulation involved in the act of taking a drug into the mouth and swallowing it may have a very decided effect on the amount of work executed on the ergograph, but even this knowledge did not lead him to adopt any control in his numerous researches on drugs.

"There is however another factor which is probably more important than either sensory stimulation or suggestion, viz., the interest and excitement produced by taking a substance when the discovery of its effect is the motive of the whole experiment. . . . If such a condition of interest as that arising from its being the first or last day of an experiment, or that resulting from the view of the weight rising as the finger contracts, can have very appreciable effects on the amount of work, it is clear that so interesting an occurrence as the administration of a drug must have a decided influence and the interest so aroused will probably be equally great whether the nature of the drug is unknown, so that there is an element of mystery in the occurrence, or whether its nature is known.

''A difficulty which arises in drug experiments is due to the practise of taking as part of the normal diet substances which have an effect on the capacity for work; and this difficulty becomes especially great when it is one of these drugs which is the subject of the experiment.

''If the use of the active substance is only given up shortly before the commencement of the experiments, there is a further danger. Even in those who only take such a substance in moderate amounts, its disuse is probably followed in some degree by the craving which is so pronounced after discontinuance of large amounts, and, slight and hardly noticeable as this craving may be, it may yet be sufficient to produce an obvious effect when the article of which the person has been deprived is administered experimentally. The effect of the substance given experimentally may be the result, not of its normal physiological action, but of the satisfaction of the craving.

''In carrying out an experiment of this kind, extending over a number of days, it is essential that all the conditions of life be kept as constant as possible. The same amount of sleep must be taken every night, the meals must be of the same kind and at the same times every day, the same amount of exercise must be taken, and the same amount of other work done.'' (Rivers, ''The Influence of Alcohol and Other Drugs on Fatigue,'' pp. 15–21.)

In the present experiment an attempt was made to take account of all the sources of error pointed out in this excellent analysis. The first error was avoided by administering, on days known only to the director, an inactive substance (sugar of milk) in the same manner and at the same time that the caffein doses were given on the remaining days. These days were not the same for all the subjects, and the result was that neither the subjects going through the tests nor the assistants who were making the measurements and records knew at any time whether the record was being made under the influence of caffein or of the inactive control substance.

In order to make the two substances completely indistinguishable and to reduce to a minimum the factor of sensory stimulation, the doses were administered in capsule form (except in a few test cases specified in the text). The caffein and the control substance presented the same appearance and neither substance was ever tasted. In some cases the capsule was taken along with a drink of water, but in most cases no such assistance was required.

The fact that the caffein days were thus unrecognizable helped to reduce the disturbing influence of excitement and interest. These factors were further reduced by running all subjects for one week on control doses only (quite without their knowledge, of course). This procedure not only served to get the subjects adapted to the conditions of the experiment before the drug doses began, but at the same time brought their performance to a more uniform practise level. Since all subjects gave up the use of all drugs three days before the experiments began, this additional week gave an interval

of 10 days in which those who had previously used caffein might become adapted to its discontinuance.

It has already been stated that all the subjects conformed to a fixed routine of time of meals, hour of retiring, amount of work, etc., so far as this was possible. And during the intensive experiment, in order to perfect this routine, all the subjects were fed at the same table and spent the whole day in the laboratory.

As a further check on the character and quality of the drug used, two commercial brands of caffein were administered and the records of administration of doses distinguished between these two brands. One of these (Schaeffer's) was identified and prepared in capsule form by the prescription department of Eimer and Amend, wholesale druggists in New York City. The other brand (Mallinckrodt's) was taken directly from the stock of the same firm. No difference was found in the action of the two brands, but the facts are given here simply as a point in the general technique of the experiment.

Supplementary Information

Through fixed routine of life on the part of the subjects, by the maintenance of uniform temperature in the laboratory, etc., it was endeavored to keep the conditions of the experiment as constant as possible. But no amount of precaution can perfectly control the conditions to which an organism is subject throughout a period of 40 days. In order to supplement these precautions, and for the personal information of the director and the convenience of the medical adviser, the results coming from the tests were further checked up by daily memoranda recorded by each subject throughout the experiment. Each individual kept a "daily health book," in which record was made of the condition of health and spirits in both forenoon and afternoon. Any unusual indications, symptoms, etc., were noted, the hour of appearance and the continuance of these indications, and their character in detail stated. This account was of course purely introspective. Any outside circumstances of an unusual or disturbing character were also reported. The approximate number of hours sleep was recorded after each night, and the quality of sleep classified as *better than usual, ordinary,* or *worse than usual.* In the case of the female subjects the beginning and end of the menstrual period were also noted. The hearts of these subjects were also examined stethoscopically at the beginning of the experiment by the medical assistant.

At the close of the complete experiment each subject was requested to reply to the following questionnaire:

QUESTIONNAIRE

(To be answered by all subjects in the book provided)

1. If before the experiment began you were accustomed to the use of coffee, tea, tobacco or any form of alcohol, have you found yourself missing or longing for any of these during the past five weeks, or have you been able to go without them without any desire or discomfort? Specify concerning each of the substances mentioned. Explain as fully as possible with reference to coffee and tea in particular.

2. On the whole do you find yourself in better or worse general condition of health, spirits and general efficiency, or do you notice any change at all? What was your weight when the experiment began? What is it now?

3. Have you at any time during the experiment read up in any kind of treatise a discussion of the supposed effects of caffein on mental and physiological processes, or in any other way made such inquiry? If so, do you think that knowledge or suggestion thus acquired has in any way influenced your health reports, or suggested special symptoms which you might otherwise have ignored? Kindly discuss this as fully as possible.

4. Will you kindly discuss each of the following tests to the best of your ability, giving as much information as possible, from your own observation and self-examination, on the questions which follow the list of tests. All that you can say here with certainty, and with no attempt at mere guessing will be much appreciated.

Color-naming Test,
Naming Opposites,
Calculation Test,
Cancellation Test,
Three-hole Test,
Weight Test,
Discrimination Reaction,
Tapping Test,
Steadiness Test.

(*a*) What particular difficulties did you have with the test in the beginning? Did you overcome these difficulties in any conscious way which you can here describe or did you just happen upon the better method quite unexpectedly and unconsciously?

(*b*) On days when you did not seem to be able to do the test as well as usual, what seemed to be the difficulty? Were you able to overcome this difficulty in any way on the days in question or did it simply stay and go away later of its own accord?

(*c*) If in the course of the experiment you came to change your method of doing the test from time to time will you kindly describe these changes, tell in what they consisted, what suggested them, and whether they proved better or worse than the original methods. Did your chief improvement come from simple practise and repetition, from observing the better methods employed by others, by deliberately trying improvements of your own, or by accidentally happening upon better methods? Explain in each case as fully and clearly as you can.

Kindly answer the above questions as fully as you can, for each of the tests.

5. Have you at any time during the experiment been able to know whether you were taking caffein or not, in your capsules, at the time of taking? This does not mean, of course, after any effect which the substance may have had begun to show itself. If so, kindly state how and when.

6. Will you kindly state at this point whether or not you have conformed to all the requirements concerning sleep, diet and regular habits which you agreed to observe throughout the experiment.

The Tests

The tests employed have been briefly enumerated in the preceding questionnaire. Each will be described in detail in the appropriate chapters of the following discussion of results. Except in the case of the size-weight illusion and in that of the steadiness test, the quality and quantity of the performance remained constant and the measurement was made in terms of the time of performance (speed). A rough record was made at the time of the test and this record was subsequently copied into a final book and a duplicate of this book made. The individual records were then averaged in various ways, and these averages placed in separate books. The averages of the various squads were also computed and recorded separately. These five methods of recording were adhered to in all the tests, by all the assistants. No assistant was aware of the records made at any time by any of the subjects in any test save those of which he was in charge. Except for a weekly announcement of the best five records in each test, the subjects themselves knew nothing concerning the record they were making except in so far as this knowledge was based on their own opinion of their performance.

Assistants

The writer's appreciation of the splendid service rendered by the following corps of assistants is here gladly expressed.

Laboratory Assistants

Leta Stetter Hollingworth, A.B., Assistant Director.
Miss L. G. Stevenson, A.B., M.D., Assistant and Medical Adviser.
Miss Margaret Tower Hart, A.B., Assistant in Experimental Psychology, Barnard College.
A. E. Chrislip, A.B., M.A., Candidate for Ph.D. in Psychology, Columbia University.
A. J. Culler, A.B., Candidate for Ph.D. in Psychology, Columbia University.
E. K. Strong, Jr., A.B., M.A., Ph.D., Fellow in Psychology, Columbia University.

CHAPTER III

Description of the Tests

I. Perception and Association

Since thinking is so largely a matter of the association of ideas, a set of three simple association tests, representing different degrees of complexity, were chosen to represent this sort of mental process in the present experiment. These three degrees of complexity are fairly clearly recognized in daily life as well as in the classical laboratory experiments. (1) The names of the objects of our experience are associated with the direct perception, through some avenue of sensation, of the objects themselves. (2) Ideas are associated with other ideas, names with other names, words with other words. (3) Tasks and situations of a more or less specific character are associated with ideas which will handle or solve them effectively.

1. *Association of Ideas and Words with Simple Objects of Experience.*

This process is illustrated by the *Color-naming Test.* The experiment is designed to measure the speed with which the name or idea can be brought to consciousness upon the sight of the object, which is in this case a color. On a white cardboard background, 26 cm. square, were pasted 100 colored squares (1 cm. sq.) of paper. Each of the colors, red, orange, yellow, brown, green, blue, violet, pink, gray and black, occurred 10 times on the card, the 100 squares being arranged in a chance order, and placed 1 cm. apart. The card was placed face downward on the table before which the subject sat. The instructions were to turn the card over at a given signal from the assistant ("Ready," and, after 2 seconds, "Go"), and to read the 10 lines of colored squares through, naming aloud, as quickly as possible, the correct name for each square. The lines were read from left to right. The assistant held a similar card and said "No" whenever a color was incorrectly named, and the reader was required to give the correct name before proceeding. The score in this test was represented by the total time (recorded by the assistant in fifths of a second) required to name through the 100 colors correctly.

In order to eliminate differences and variations due to possible unfamiliarity with color names, each subject was given a few preliminary trials in which the proper simple name was associated with

each of the colors. In order to eliminate any memory effect which might have come from naming the series over repeatedly in the same order, the plan was followed of turning the card about (90 degrees) on the second trial of the day. This was done for each of the five trials on a given day, the last order thus being the same as the first, for any given day, the three other orders having meanwhile intervened. This practise was followed only during the first four weeks, in which only five tests were made daily. The same sequence of orders, *A, B, C, D, A,* was followed on all the days. During the intensive experiment (three days, 15 trials daily) no changes in order were made, order *A* being used throughout. As a matter of fact very little memory effect was found to be present. At the close of the experiment all the subjects were tested for memory effect by first asking them to recite the various orders from memory, and then by giving them the first three names of a given order and asking for the following colors. In spite of the fact that the colors had been named 220 times by each subject during the experiment, no one was able to do more than give a few groups of three or four colors in their proper order, and even the proper location of these groups in the series or on the card was impossible. The assistant who had gone over the test about 3,300 times knew scarcely more about the order of the colors than did the subjects themselves. There had been of course no intention to memorize, in the test, and the result affords an interesting suggestion with respect to the part played, in memory processes, by the "determination to learn."

In the beginning of the experiment this test constitutes a measure of the individual's familiarity with colors and color names, and his accuracy of color discrimination, as well as the absolute quickness of the association process involved and the facility of articulation. As the experiment progresses it affords a measure of the individual's ability to improve by practise, his degree of interference as shown by the tendency of a preceding idea to inhibit or interfere with the correct perception and expression of the next stimulus. It also measures the regularity of his performance and his susceptibility to fatigue. The Color-naming Test has been recommended by Cattell and Farrand ("Physical and Mental Measurements of the Students of Columbia University," *Psychological Review,* November, 1896, p. 642) and has been used for several years in the Columbia Laboratory (see Wissler, "The Correlation of Mental and Physical Tests," *Psychological Review,* Monograph Supplements, Vol. III., No. 6, 1901).

2. *Association of One Idea with Another Specific Idea.*

This process is represented in our experiment by the test of *Naming Opposites.* On a cardboard which lay face downward before the subject was the following list of 50 adjectives, typewritten, in two parallel vertical columns of 25 each. The instructions were to turn the card over at the given signal and proceed down the list, naming aloud the *opposite* (in meaning) to each word in the order in which it occurred. The time required for this performance was measured by the assistant in fifths of a second. In order to make the test one of association time rather than of linguistic knowledge, such errors as giving an adverb as the opposite of an adjective, etc., were pointed out. If an unacceptable word was given the assistant exclaimed "No" and the subject was required to give a correct opposite before proceeding. The same fifty words were used at each trial, but each time they occurred in a new and chance order, determined by shuffling a deck of cards on each of which one of the words was written, and making out the typewritten list in the order in which the cards turned up.

Name the Opposite of Each Word, in Turn, as Quickly as Possible

loud	fertile	vague	rash	tragic
slovenly	wise	ancient	ripe	graceful
innocent	masculine	foreign	dangerous	shallow
broad	beautiful	timid	prompt	drunk
public	sacred	harmonious	fickle	cloudy
stale	brief	coarse	aristocratic	sharp
sickly	defective	noisy	peculiar	stormy
gay	helpless	past	talkative	idle
soothing	expensive	hostile	attractive	savage
cowardly	doubtful	gentle	victorious	lazy

These words were chosen from a list of 200 which had been previously applied by Professor R. S. Woodworth to many individuals, measuring the time required to name the opposite of each word when the words were presented separately instead of serially. The above list of 50 adjectives were chosen because they all fell into the class of "moderately difficult," their average times ranging from 2 to 5 seconds.

The *Opposites Test* is a much used one in experimental and educational psychology and has been found to correlate to a fairly high degree with other tests designed to measure "mental ability."[1] As the test is conducted the mental process is essentially one of con-

[1] See, for example, F. D. Bonser, "Reasoning Ability of Children," Columbia Contributions to Education, No. 37.

trolled association, with the quality and quantity of the performance constant and the time variable. To think of the exact answer to a question, the precise address of a friend, the exact shade of meaning of a word, delicate distinctions of connotation in the use of language, all involve processes of controlled association, processes in which, from many ideas which the stimulus calls up, the one appropriate idea is recognized, selected and expressed, while the false or inadequate ideas are repressed. Hence this test indicates the ability of the individual to select the appropriate response from the host of ideas which follow in the wake of a stimulus word. It is an index of speed, accuracy, linguistic feeling, and of the ability to repress useless or irrelevant ideas. At the same time it shows the ability of the individual to improve by practise in such a performance, as well as the regularity of that performance. It is a test of association processes, but of association processes of a considerably more complex kind than those involved in the *Color-naming Test.*

3. *Association of an Idea with a Specific Task or Situation.*

A still more complex stage of association is found when a specific task or situation calls for an appropriate and immediate response. The ideas themselves are not given. The individual must provide his own ideas and images, and may manipulate them in his own way, but he must in some way come out of the process with the right response. It is thus a case of evolving an idea to meet an unexpected situation. The question will be, how quickly can the individual manipulate his mental processes so as to call up the right idea in his consciousness and set up movements of articulation which will express to an onlooker the result of his thinking. The *Calculation Test* was chosen to represent this type of association. A card was prepared containing 50 two-place numbers between 20 and 80, all numbers ending in 0 being omitted.[2] These 50 numbers occurred in a random order, and each number occurred but once in a list. The subject was required to turn the card face upward at the starting signal, and, without the aid of any graphic device, to add 17 mentally to each of the numbers on the card. The answer was to be spoken aloud and was checked up by the assistant who held the key card containing the correct answers. In case of a false calculation the subject was required to correct his answer before proceeding. The time required to perform the 50 additions was measured in fifths of a second. At the next trial the same numbers appeared on the card,

[2] For demonstration of the advantages of this type of calculation test see F. L. Wells, ''Standard Tests of Arithmetical Associations,'' *Journal of Philosophy, Psychology and Scientific Methods*, Vol. IV., No. 19, September 12, 1907, pp. 510–512.

but in a new random order. In the intensive experiment the list was increased to 75 numbers, 25 of the original ones thus appearing twice in the series. During the experiment of the last week, 100 numbers were used, each of the original 50 thus appearing twice in random order.

II. *Discrimination, Attention and Judgment*

In order to determine the influence of caffein on such higher mental process as sensory discrimination, attention and judgment, three tests were employed, the familiar size-weight illusion, a cancellation test, and a series of measurements of choice-reaction times to color stimuli.

(*a*) *Discrimination.*

In order to determine whether or not such release of central control as might conceivably be produced by caffein would result in a correspondingly increased susceptibility to illusion, what is technically known as the size-weight test was employed.[3] From a group of 14 cylindrical weights of the same size (2.5–3.5 cm.) but differing in weight (from 15 to 80 grams, by increments of 5 grams) the subject was required to select the one which seemed to him equal in weight to a constant standard block which was several times the size of the weights constituting the series. This standard was a 55 gram weight, cylindrical in form, 4 cm. in height and 7 cm. in diameter. The weights rested on a flat cushion made of heavy towelling, and were lifted with thumb and forefinger, the weights being always compared with the standard and never with each other. The normal tendency here is to select a weight which is much lighter than the standard block, usually less than half as heavy. The subjects were all unaware of the presence or character of the illusion and were unable to identify the weight selected on previous occasions.

The normal illusion was present with all subjects, and increased in amount as the experiment progressed. But comparison of the caffein days with the control days shows no difference in the amount of the illusion. It is possible that this was due to the fact of the 5 gram differences between the weights. No difference smaller than 5 grams could thus be detected, although some influence might have been disclosed had the weights been graded by smaller amounts of difference. Since this test failed to yield any result it was discontinued after the first four weeks and was not used during the intensive experiment nor during the final experiment of one week. Since the results of this test have no apparent bearing on the character of the caffein influence they are not included in this monograph.

[3] See E. W. Scripture, "The New Psychology," pp. 272–282.

(*b*) *Attention and Discrimination Combined.*

The Cancellation Test.—Various forms of the cancellation test have been employed in investigations designed to measure the degree of attention, distraction, discrimination, fatigue, etc. In the various forms in which the test has been used the task has been that of crossing out all the cases of a given letter, figure, word, or symbol, or some combination of these occurring on a printed sheet along with other material from which the given symbols have to be discriminated. In these tests the measure usually consists of the time required to complete a given amount of cancellation correctly. It is assumed that the best speed will be made under conditions of maximal attention and that any tendency to distraction will be reflected in the speed of performance provided the quantity and quality of the work remain constant.[4]

Such a cancellation test was used in section 1 of the present experiment. A printed sheet contained each of the digits from 0 to 9, repeated 100 times. The sheet contained 20 lines, and each digit occurred 5 times in each line. Aside from this regularity the distribution represented a chance order. The subject was required to begin at the upper left hand corner and to cross out with a pencil all of the 2's, 3's, 5's, 6's, or 8's. A different digit was used at each of the five daily trials, the order being that in which the digits are named above. Obviously these five digits can not be distinguished with equal ease and speed from the background of the printed sheet, so that the absolute speeds made at different times during the day can not be compared except in terms of ratio. But, contrasting caffein days with control days, either the totals or the separate trials at the various hours may be compared independently of the digit used at the time. In order that the quantity and quality of the performance be kept constant throughout, the subjects were informed that each digit would occur five times in each line, and were instructed to mark off all five in a given line before proceeding to the next one.

This test is a difficult one to handle in practise because the individual subjects will differ in their acuity of vision and in their susceptibility to the eyestrain which the test easily induces. To use the total number of lines (20) on the sheet was found to be impracticable. During the preliminary week only ten lines were used, and after that the number was increased to fifteen lines, at which it remained throughout the remaining three weeks in which caffein was administered. One of the subjects (No. 1) of the control squad was unable to continue the test after the first few days because the eye strain involved rendered its performance painful.

[4] See G. M. Whipple, "Manual of Mental and Physical Tests," pp. 254–270.

(c) *Discrimination and Choice Reaction.*

This test was made with the aid of the "Columbia" chronoscope, a form of the pendulum chronoscope designed by Professor Forbes, of the department of physics, Columbia University. A spring release key which presented a colored disc in the exposure aperture, set free at the same instant a pendulum which swung across a scale graded in *sigma* until the pressing of a key on the part of the subject completed an electric circuit through a pair of magnets. These magnets caught the pendulum in its swing and held it before the graded scale until the record could be read by the operator. The subject was instructed to react to the appearance of a red disc by pressing, as quickly as possible, with the forefinger of the left hand, a telegraph key in circuit with an electric buzzer. On the appearance of a blue disc he was to react in a similar way on a key in his right hand. This key was in circuit with the chronoscope. Preliminary trials practised him in this process. At the same time occasion was taken to warn him against making false responses, which thereafter, with most subjects, occurred only infrequently. Record was, however, made of all such false reactions. Ten correct reactions to the blue disc were secured at each sitting, and interspersed with these (in a chance order determined by the appearance of reds or blacks in a shuffled deck of cards) from five to ten reactions to the red disc. The number of reds was varied in this way in order to prevent anticipatory reactions. The ten reactions for each sitting were averaged and their mean variation from the average computed. These two figures constituted the record for the given trial. The standard precautions concerning ready signal and interval were observed, and the mechanism concealed from the subject.[5]

III. *Motor Tests. Steadiness, Speed and Coordination*

1. *The Steadiness Test.*—The steadiness with which the individual could hold the outstretched arm was measured in the following manner: A metal rod 2.5 mm. in diameter was held in a hole formed in a brass plate. During the first four weeks the diameter of this hole was 6 mm. and during the rest of the experiment 4.5 mm. Every contact of the rod with the sides of the hole was registered by an automatic electric counter. The task was to stand unsupported by the table on which the apparatus was placed and to hold the rod in the hole for one minute with as few contacts as possible. The test

[5] For significance and technique of the discrimination reaction experiment see J. McKeen Cattell, *Philosophischien Studien,* 1886, III., p. 460; Ladd and Woodworth, "Physiological Psychology," pp. 470–499.

thus shows the general stability of the nervous system and the precision with which one set of muscles can be delicately balanced against another. Since all movements beyond the slightest tremor are recorded by the apparatus (especially when the smaller hole is used) any increase in trembling, twitching or general nervousness is easily detected so far, at least, as the horizontal plane is concerned. (For discussion, of the technique and significance of the steadiness test see Ch. V; also cf. Whipple, "Manual of Physical and Mental Tests," pp. 123–127.)

2. *The Tapping Test.*—Using the above described metal rod held in the right hand, the subject executed as rapidly as possible 400 taps on a solidly planted metal base. Each tap was recorded by the electric counter and the time required for the first 200 and the last 200 taps was measured in fifths of a second with a stop watch. This is the simple form of the tapping test used by Bryan and others, and is much too crude for an intensive study of the course of fatigue or the regularity of the performance. But more elaborate methods were impracticable in the present experiment, and the investigation in this test was limited to the influence of caffein on the total time required to make the given number of taps. In section 3 the number of taps to be made was raised to 500. (For full discussion of the technique and significance of the tapping test see F. L. Wells, *American Journal of Psychology,* Vol. XIX., pp. 345–358 and pp. 437–483; Vol. XX., pp. 38–59 and pp. 353–363. Also Whipple, "Manual of Mental and Physical Tests," pp. 100–115.)

3. *Coordination. The Three Hole Test of Combined Accuracy and Speed.*—The three hole test, as it is technically called, includes, along with the factors of steadiness and speed, which are essentially motor or physiological, the more strictly mental factor of coordination. An oak plate tilted at an angle of 45 degrees to the base board, contained three brass-line holes arranged in the form of an equilateral triangle, about 8 cm. apart. Contact of the metal rod with the bottom of a hole made an electrical connection which was recorded by the automatic counter. The subject held the rod in his left hand because the right hand had just been used for the tapping test. The task was to insert the rod into each of the three holes successively as rapidly as possible until 100 insertions had been made. The time required for this process was measured by the assistant with the stop watch in fifths of a second. Success in this test requires not only that a single set of muscles be brought into harmonious action, as in the tapping test, but also that several sets be coordinated with each other, under the guidance of a visual impression. In making the movements most of the muscles of the arm are in-

volved, as are the external muscles of the eye and its mechanism of accommodation. Arm movements must be coordinated with eye movements as accurately and at the same time as quickly as possible. The test is thus a measure of combined accuracy and speed—it measures the efficiency with which such stability and activity as are available can be brought under the control of a purely mental effort—the coordination of a complex set of activities focused on the accomplishment of a single and definite task. The series of motor tests, of which this is the third, seem to afford significant indices of general motor capacity, and usually reveal clear cut individual and sex differences in inertia, speed, accuracy, fatigue and rate of improvement.

CHAPTER IV

The Influence of Caffein on the Tapping Test

The tapping test seems to have been little used in experiments on the influence of drugs. The motor tests usually employed have been chiefly ergographic or dynamometric in character and designed to measure the force and number of contractions of which a muscle or set of muscles is capable when working against a load. The tapping test measures rather the speed with which unloaded muscles can execute successive contractions of a narrow range.

The following individual curves show in a preliminary way the stimulating effect of caffein on this performance. The curves show in each case the time required to execute 400 taps. The broken line record was made on control days and the solid line record on caffein days, the two kinds of days alternating regularly. The first pair of each set show forenoon records made before any dose whatever was taken, hence are both control or normal records. In all three cases there is no difference between these two curves. Both show practise throughout the course of the experiment, but caffein days show no superiority over control days. The second pair of curves in each set shows the records made in the afternoon, some time after the capsule had been taken. The superiority of the caffein days is here apparent. Stimulation is present for all amounts.

In the following curves the first pair for each subject gives records before the dose and the second pair the records after the dose. The solid line represents caffein days and the broken line control days. Unit, the time required for 400 taps.

These three individual records are typical of the behavior of the other members of the caffein groups. Considerable individual differences are shown, varying from no very marked effect at all (as with Subject VI.) to the great stimulation shown by Subject XIV. But in no case is there any evidence of continued retardation after caffein. The general tendency can then be more safely discussed on the basis of the squad averages, which will show under each condition the mean effect on the several individuals comprising the squads.

In giving these averages the mean variations would be of no value since there are but 3–5 individuals in each squad and the mean variation of their average record at a given test would simply reflect their individual differences. Since the abilities of the various indi-

viduals, though constant in themselves, are considerably different from each other, the M.V.'s of the averages will always be much larger than the difference produced by the use of caffein on any one person's record. Hence it seems most informative and economical of

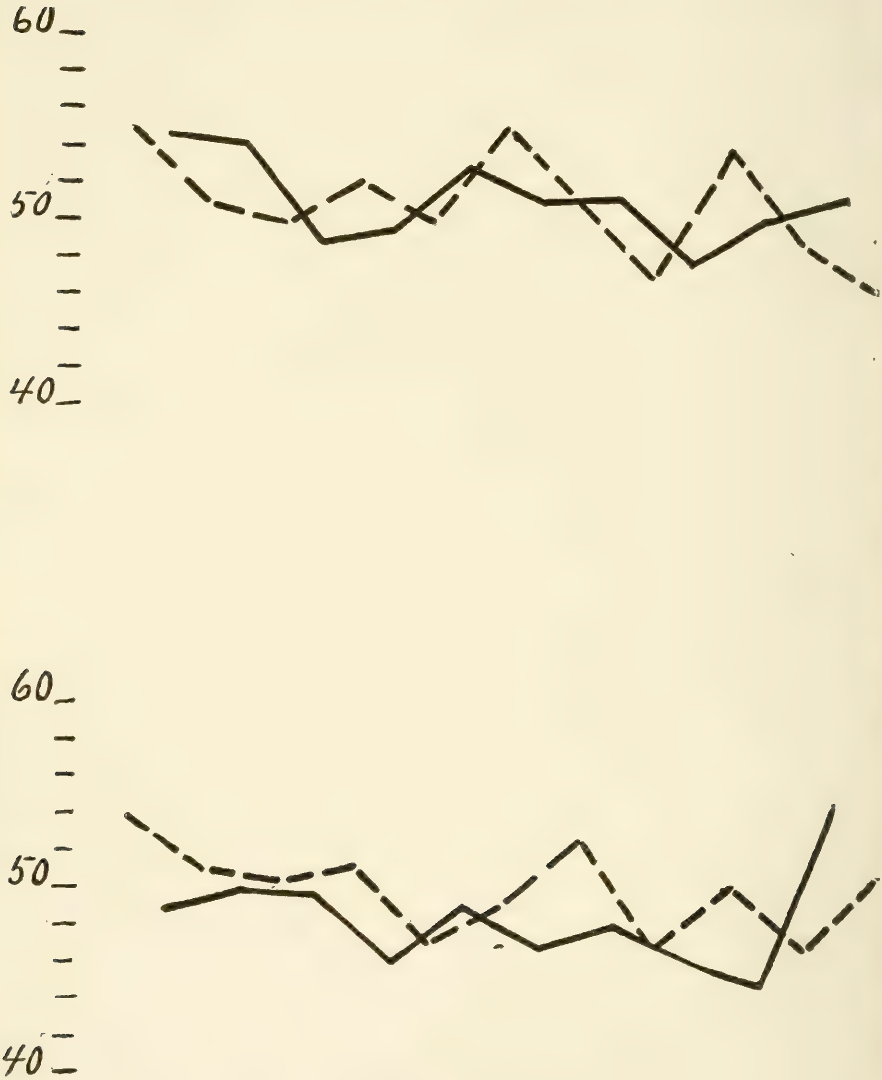


FIG. 1. Subject VI., 12:00 and 3:10.

space to treat the total time required by the squad at any given test, rather than the time required by each individual, as a unit of performance. There is then no question of M.V., since the total squad

time (divided by the number of persons in the squad in order to reduce the magnitude of the numbers) constitutes a single measurement. This method of treating the squad as an individual thus tends to minimize the effect of variations due to foreign factors and to compensate for the personal variations of the individual subjects.[1]

Fig. 2. Subject X., 12:00 and 3:10.

In the tables that follow are given the squad averages for each hour of the day on all the days of Experiment *A*, except that the first week of practise, in which no caffein was administered, is not included. The hour of the dose is also indicated in each table, *s* signifying sugar and a number of grains (as 3 gr.) signifying a

[1] This plan is adhered to in reporting the other tests as well.

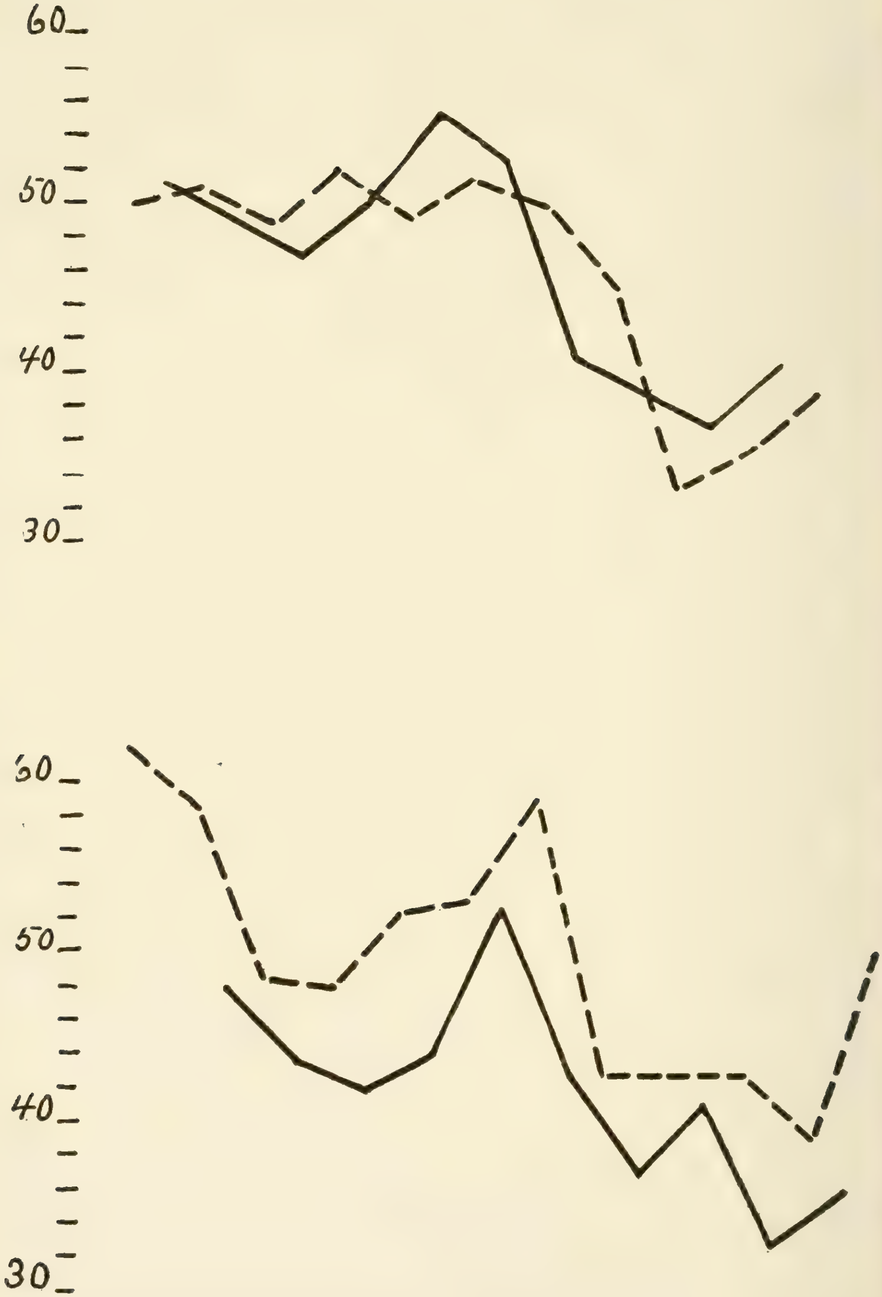


FIG. 3. Subject XIV., 10:00 and 5:30.

dose of caffein. The records for caffein days are in italics, but it should always be borne in mind that it is only after the hour of the dose that the italics indicate performance under the influence of caffein. If the caffein influence holds over until the following day the effect must be looked for in the characteristics of the unitalicized records of the day in question.

In Experiment *A*, Squad I., is the control squad who took only sugar doses for 25 successive days, the last 18 of which are shown in Table II. But in treating the data from this squad the records are handled just as though the caffein days for Squads III. and IV. had also been caffein days for Squad I. Comparison of these pseudo-caffein days with the remaining days, in Squad I. might be expected to indicate the likelihood that any result found in the case of the other squads is only a chance result.[2]

TABLE II

TAPPING. SQUAD I. (CONTROL), EXPERIMENT *A*

Control doses only. Records treated as though odd days were caffein days and only even days control

	February													
Hour	10	11	12	13	14	15	16	17	18	19	20	21	22	23
7:45	58.5	*55.9*	58.0	*56.4*	55.8	*53.9*	54.7	*54.5*	53.5	*54.8*	55.9	*52.5*	55.6	*52.4*
10:00	56.6	*57.6*	57.9	*55.1*	55.8	*54.8*	56.4	*52.1*	53.7	*54.0*	52.6	*54.3*	51.3	*52.2*
12:00	58.7	*57.9*	58.0	*55.7*	55.2	*56.9*	54.3	*55.1*	52.6	*55.5*	51.7	*51.8*	53.4	*51.5*
1:00	Dose, sugar capsules only, daily.													
3:10	59.7	*56.1*	56.2	*55.3*	52.7	*55.0*	56.3	*54.1*	53.6	*53.1*	54.1	*51.8*	57.7	*50.9*
5:30	64.9	*57.3*	57.2	*55.8*	56.7	*56.4*	53.8	*52.3*	54.1	*51.4*	51.3	*51.0*	51.2	*52.1*

Turning now to the examination of Tables II.–V., reading along the horizontal lines, gives the record for any given hour of the day, on each day of the experiment. The italicized caffein-day records can thus easily be compared with the control records for the same hour on both the preceding and the following days. Reading down the vertical columns gives the successive records made on any single day. Comparisons of records made before and after the dose are thus easy to make.

With the data presented in this form only it is not easy to discuss

[2] The writer regrets the impossibility of presenting in the form of curves all of the vast amount of data treated in this monograph. It would greatly facilitate comparison on the part of the reader, but is quite out of the question because of the amount of space that would be required. Even the inclusion of the curves would not do away with the desirability of presenting the actual figures in tabular form. These figures will always be given in such shape that any set of results which the reader wishes to examine more minutely may easily be platted from the data given in the tables.

TABLE III

TAPPING. SQUAD II., EXPERIMENT *A*

Caffein three successive days, alternating with three control days

	February																		March	
Hour	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	49.5	49.2	*48.5*	*48.3*	*47.9*	43.7	45.1	*46.4*	*43.7*	*46.1*	46.3	49.8	43.3	*41.8*	*42.1*	*42.1*	42.5	40.7	40.9	*42.8*	42.2
10:00	48.5	48.1	*51.7*	*49.0*	*46.1*	46.6	45.2	*43.9*	*43.9*	*44.9*	43.5	46.6	45.8	*42.7*	*44.7*	*43.0*	41.5	40.7	40.4	*41.6*	45.9
10:30	S	S	*1 gr.*	*1 gr.*	*1 gr.*	S	S	*2 gr.*	*2 gr.*	*2 gr.*	S	S	S	*4 gr.*	*4 gr.*	*4 gr.*	S	S	S	*6 gr.*	S
12:00	49.0	48.0	*47.4*	*47.3*	*46.8*	45.9	47.2	*42.8*	*43.7*	*46.7*	44.5	43.9	46.9	*38.9*	*46.3*	*44.2*	41.3	40.0	39.1	*40.7*	45.3
3:10	52.3	48.8	*46.8*	*45.2*	*47.9*	46.1	44.2	*44.7*	*43.0*	*44.5*	45.3	46.1	44.2	*43.5*	*44.2*	*41.6*	42.3	40.9	40.9	*38.3*	43.2
5:30	46.3	48.4	*48.2*	*46.0*	*46.7*	44.3	43.5	*43.0*	*43.0*	*45.3*	39.9	43.0	41.2	*40.5*	*41.1*	*42.1*	43.0	42.1	41.0	*40.9*	46.4

TABLE IV

TAPPING. SQUAD III., EXPERIMENT *A*

Caffein on alternate days, with lunch

	February																		March	
Hour	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	58.0	*57.0*	56.9	*52.9*	59.5	*58.0*	53.2	*54.1*	54.7	*56.6*	55.3	*56.5*	48.3	*54.1*	54.1	*47.9*	49.3	*48.6*	48.7	*54.2*	55.7
10:00	56.9	*58.9*	53.8	*55.3*	55.2	*54.1*	54.1	*53.4*	55.2	*56.6*	55.1	*54.5*	49.7	*46.0*	48.3	*45.2*	43.2	*43.3*	44.0	*45.3*	44.4
12:00	57.9	*67.1*	54.7	*55.0*	57.6	*57.7*	54.5	*51.9*	56.4	*56.7*	52.9	*45.8*	50.9	*47.9*	45.3	*46.9*	50.7	*42.5*	45.3	*48.3*	50.3
1:00	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	60.0	*58.7*	54.9	*55.6*	53.4	*51.3*	51.9	*50.1*	53.8	*50.6*	48.1	*51.8*	49.3	*45.8*	44.9	*43.9*	46.7	*43.1*	50.3	*44.5*	50.9
5:30	60.5	*48.7*	52.9	*53.7*	52.9	*51.2*	52.7	*50.9*	53.7	*53.0*	52.9	*45.9*	48.3	*42.5*	47.4	*45.9*	53.3	*41.5*	46.2	*42.9*	49.5

TABLE V

TAPPING. SQUAD IV., EXPERIMENT *A*

Caffein on alternate days, about 2 hours after lunch

	February																			March	
Hour	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	56.6	*55.4*	55.0	*54.9*	56.0	*53.1*	53.5	*51.6*	51.7	*53.8*	56.6	*50.9*	49.5	*53.9*	52.0	*50.6*	52.0	*53.1*	51.8	*51.0*	50.9
10:00	57.8	*51.8*	54.8	*54.8*	53.7	*55.8*	53.1	*54.9*	51.9	*52.3*	51.4	*51.4*	49.4	*51.4*	51.4	*51.2*	48.2	*52.4*	53.2	*50.8*	53.7
12:00	56.8	*56.9*	55.7	*55.0*	54.6	*52.3*	53.9	*54.2*	53.1	*53.1*	53.4	*52.8*	51.5	*51.9*	53.8	*51.4*	52.2	*52.1*	51.8	*51.3*	53.5
1:45	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	60.2	*53.4*	54.5	*54.7*	55.1	*53.8*	53.2	*49.9*	52.9	*51.0*	52.4	*50.6*	51.3	*50.0*	49.4	*48.1*	52.4	*49.3*	50.0	*51.7*	51.8
5:30	55.6	*55.1*	55.8	*55.5*	54.3	*53.8*	53.0	*49.9*	53.7	*49.5*	50.2	*51.1*	51.0	*51.8*	50.7	*48.0*	51.4	*47.9*	52.5	*49.0*	51.0

results, although the graphs which it is impossible to include show them clearly. In order to concentrate the material and thus facilitate comparison of control days with caffein days the following method of combining the records has been adopted. Although in most of the tests the first week's practise (35 trials) which is not included in these tables brought all the individuals fairly well toward a practise level of performance, there is still considerable improvement during the three weeks (105 trials) shown in the tables. Hence it is unfair to compare caffein record with either the control record of the day before or with that of the day after, alone. Nor is it satisfactory to rely on the change in performance at later trials on the same day, for here such factors as diurnal variation, fatigue, etc., enter. A more satisfactory plan is to compare each caffein record with the average of the records for the same hour on the preceding and following days. This average would give a calculated control value for the intermediate record. Comparison of this calculated measure with the actual record made on any given day will indicate the presence or absence of caffein influence. Thus on Feb. 19 at 1:45 P.M., Squad IV. took 3 gr. of caffein. At 3:10 their record was 51.5 sec. On the preceding day (Feb. 18) at the same hour their record was 52.9 sec., and on the following day (Feb. 20) 52.4 sec. The calculated record for Feb. 19 is then $(52.9 + 52.4)/2$ or 52.65 sec. But the actual record was only 51.50 sec. This is then to be interpreted as signifying 52.65–51.50 or 1.15 sec. stimulation produced by the 3 gr. of caffein. In the same way the calculated record for Feb. 21 at 3:10 for the same squad is $(52.40 + 51.30)/2$ or 51.85 sec. The actual record, after another 3 gr. dose of caffein, was only 50.60. This shows 51.85–50.60 or 1.25 sec. stimulation. Further, averaging the calculated records for the 19th and 21st we get $(52.65 + 51.85)/2$ or 52.25. But the average of the actual records made on those days is only $(51.50 + 50.60)/2$ or 51.05. The average effect of 3 gr. of caffein under the circumstances prescribed for this squad, based on two observations, is then 52.25–51.50 or 1.20 sec.

Such calculations have been made for each caffein dose for all four squads, and the average influence of the various-sized doses (two trials for each dose with Squads I., III. and IV., and three trials for each with Squad II. except for 6 gr.). These results are presented in Tables VI.–IX. In these tables the records for caffein days (italicized) parallel the records for control days and the tables show the average results of the several trials for each dose. A third column under each hour of the day indicates the difference between the two records (control days minus caffein days). After the dose has been taken a + in this column means stimulation and a — means

TABLE VI

SQUAD I. TAPPING

7:45 Sug. Av.	7:45 Caf? Av.	Dif.	10:00 Sug. Av.	10:00 Caf? Av.	Dif	12:00 Sug. Av.	12:00 Caf? Av.	Dif.	1:00 Dose	3:10 Sug. Av.	3:10 Caf? Av.	Dif.	5:30 Sug. Av.	5:30 Caf? Av.	Dif.
57.6	*56.2*	+ 1.4	57.1	*56.4*	+ 0.7	57.5	*56.8*	+ 0.7	S	56.3	*55.7*	+ 0.6	59.1	*55.9*	+ 3.2
54.7	*54.2*	+ 0.5	55.6	*53.5*	+ 2.1	54.2	*56.0*	— 1.8	S	55.8	*54.6*	+ 1.2	54.7	*54.4*	+ 0.3
55.3	*53.6*	+ 1.7	52.6	*54.2*	— 1.6	52.4	*53.6*	— 1.2	S	55.4	*52.6*	+ 2.8	51.8	*52.1*	— 0.3
54.3	*52.5*	+ 1.8	53.7	*51.6*	+ 2.1	52.2	*51.5*	+ 0.7	S	54.1	*51.6*	+ 2.5	51.2	*52.3*	— 1.1

TABLE VII

SQUAD II. TAPPING

Dose	Average 1st and 2d	Average Per Cent. 3d and 4th	Average Per Cent. 5th	Average Per Cent. 3d, 4th and 5th	Cases
Sugar	45.0	100.5	97.7	99.1	11
1 gr.	48.6	98.5	96.5	97.5	3
2 gr.	44.7	97.5	96.7	97.1	3
4 gr.	42.7	100.9	96.4	98.6	3
6 gr.	42.2	94.3	96.9	95.6	1
Caffein Av. ..	44.6	97.8	96.6	97.2	10

TABLE VIII

SQUAD III. TAPPING

7:45			10:00			12:00			1:00	3:10			5:30		
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
57.9	*55.0*	+ 2.9	55.7	*57.1*	— 1.4	55.0	*61.0*	— 6.0	1 gr.	56.2	*55.5*	+ 0.7	54.8	*51.2*	+ 3.6
55.2	*56.1*	— 0.9	55.4	*53.8*	+ 1.6	56.1	*54.8*	+ 1.3	2 gr.	54.0	*50.7*	+ 3.3	53.0	*51.1*	+ 1.9
53.4	*56.5*	— 3.1	53.8	*55.5*	— 1.7	53.3	*51.3*	+ 2.0	3 gr.	49.8	*51.2*	— 1.4	52.0	*49.5*	+ 2.5
51.5	*51.0*	+ 0.5	47.4	*45.6*	+ 1.8	48.1	*47.4*	+ 0.7	4 gr.	46.5	*44.9*	+ 1.6	49.2	*44.2*	+ 5.0
50.6	*51.4*	— 0.8	43.9	*44.3*	— 0.4	47.9	*43.9*	+ 4.0	6 gr.	49.6	*43.6*	+ 6.0	49.0	*42.2*	+ 6.8

TABLE IX

SQUAD IV. TAPPING

7:45			10:00			12:00			1:45	3:10			5:30		
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
55.7	*55.2*	+ 0.5	55.3	*53.3*	+ 2.0	55.8	*56.0*	— 0.2	1 gr.	56.1	*54.1*	+ 2.0	55.4	*55.3*	+ 0.1
53.7	*52.4*	+ 1.3	53.0	*55.4*	— 2.4	53.9	*53.3*	+ 0.6	2 gr.	53.7	*51.9*	+ 1.8	53.6	*51.9*	+ 1.7
53.7	*52.3*	+ 1.4	51.1	*51.8*	— 0.7	53.1	*52.9*	+ 0.2	3 gr.	52.3	*50.8*	+ 1.5	51.3	*49.8*	+ 1.5
51.4	*52.3*	— 0.9	50.1	*51.3*	— 1.2	53.1	*51.7*	+ 1.4	4 gr.	50.7	*49.1*	+ 1.6	51.0	*50.2*	+ 0.8
51.6	*52.1*	— 0.5	52.1	*51.6*	+ 0.5	52.3	*51.7*	+ 0.6	6 gr.	51.1	*50.5*	+ 0.6	51.9	*48.5*	+ 3.4

retardation. But it must be remembered that before the dose has been taken the reverse holds, since the morning records of caffein days follow upon the sugar doses of the day before, and are properly control records, while the morning records of the control days are subsequent to the caffein doses of the preceding day. The tables indicate in separate columns the time and amount of the dose.[3]

Table VI. is the record of Squad I., the control, to which sugar only was given. In the table only the even days are treated as sugar days, the odd days being handled as *if they had been* true caffein days. After the sugar capsule at 1:00 P.M. there is what appears to be a stimulation at the 3:10 period for all doses. But examination of the individual records shows that this appearance is due entirely to the brisker performance of Subject XV., a woman who traveled through the tests in company with two members of Squad IV. who had really had caffein, and show considerable stimulation. There is little doubt in the mind of the experimenter that this subject was spurred on in the tapping test by the suggestions of greater speed coming from the two other subjects in the room. This appearance is then spurious and it is the only evidence of stimulation present and does not vary with the size of the dose. At 5:30 the differences between the calculated and the actual records are small and balance each other as to sign.

Squad II. took similar doses at 10:30 A.M. on each of three successive days. Hence the method of comparing actual with calculated record can not easily be applied. Instead, the 7:45 and 10:00 trials of each day have been averaged and treated as the normal records for the respective days. With these normals have been compared the average of the two trials after the dose (12:00 and 3:10) and also the final trial of the day (5:30). These latter records have been transformed into terms of per cent. of the normal for the corresponding day. Table VII. summarizes the results of this computation. Thus the average normal performance of the 11 control days is 45 sec. The average of the two trials following the sugar capsule is 100.5 per cent. of this normal, showing slight fatigue. Comparing this with the similar records after the caffein doses, we find a tendency to stimulation instead of fatigue in the latter cases, the average time of all four amounts (1, 2, 4, 6 gr.) being only 97.8 per cent. of the normal performance. With the exception of the 4 gr. doses (double the amount ever taken before and following abruptly upon three sugar days) the stimulation increases with the size of the dose. For these 4 gr. doses, however, there is slight retardation (.9 per cent.) instead. The final trials for the control days average 97.7 per cent. of the

[3] This method of presenting the data is also followed in the later chapters.

normal, indicating a tendency to superior performance at the end of the day. (This is a characteristic of the tapping test which has an interesting bearing on the problem of diurnal variation, but the matter can not be taken up in the present study.) After the caffein doses, however, this superiority of the evening performance is over 1 per cent. greater, and without exception, the average record being only 96.6 per cent. of the normal. Averaging the three tests after the dose and comparing this measure with the normal morning record for the various types of days, shows a superiority of 2 per cent. in favor of the caffein days, and shows stimulation for all sizes of caffein dose, the amount of stimulation on the whole increasing slightly with the size of the dose (97.5, 97.1, 98.6, 95.6). We may conclude then that caffein taken in the middle of the forenoon increases the speed of performance by this squad, yielding an average of 2 per cent. superiority over the speed of control days, the actual amount of stimulation depending, in part at least, on the size of the dose.

Table VIII. gives the results secured from Squad III., computed in the same way as those for Squads I. and IV. The dose here was taken at 1:00 P.M., during or immediately at the close of the lunch period. At the 3:10 test there is already evidence of stimulation after all but the three grain dose. By 5:30 the increase in speed is pronounced and increases with the size of the dose from 1.9 sec. at 2 gr. to 6.8 sec. at 6 gr. Turning to the 7:45 and 10:00 o'clock trials we find practical balance, the differences between control days and caffein days being slight and as often positive as negative. There is then, so far as these data are concerned, no evidence of any after effect, either of stimulation or retardation. We may say then that caffein administered along with food substance at lunch time produces stimulation, which (except for the 6 gr. dose) is not especially marked until the 5:30 period, when it is considerable and varies directly with the size of the dose. The amount of stimulation found here (an average of 4 sec. or about 8 per cent.) is much greater than that found with Squad II. to whom the caffein was administered in the middle of the forenoon. There is no evidence of any secondary effect up to noon of the next day, which is as far as Experiment *A* is able to trace the influence of the dose.

Table IX. shows the data from Squad IV., to whom the dose was administered on an empty stomach, two hours after lunch hour. At the 3:10 test there is at once clear evidence of about equal stimulation for all doses of caffein. At 5:30 this stimulation is still greater and tends to vary in amount directly with the size of the dose, from .1 sec. for 1 gr. to 3.4 sec. for 6 gr. On the next morning at 7:45, 10:00 and 12:00 the differences between caffein days and control

days are small and vary in sign, with no clear indication of an after effect of any kind.

The above method of treating the data from Squads I., III. and IV. proceeds on the assumption that the proper standard of performance with which the results after caffein should be compared, is the record made at a corresponding time of day after a control dose. But this is not the only standard available in our experiment. Since

TABLE X

TAPPING. SQUAD I., EXPERIMENT *A*

Ratios of performance after dose to performance before dose

Subject	Control Av.	M.V.	Pseudo-Caf. Av.	M.V.
1	1.011	.014	.985	.034
4	1.002	.057	1.015	.046
7	1.058	.101	.985	.038
15	.986	.030	.983	.044
Average	1.014	.050	.992	.040

trials were made, on both control and caffein days, both before and after the dose, it is possible to compare the work done after the dose, on any given day with the work done on that same day before the administration of the dose. Such a comparison should afford a valuable check on the conclusions based on the comparison of records after control doses with those after caffein doses. In fact such a compari-

TABLE XI

TAPPING. SQUAD III., EXPERIMENT *A*

Ratios of performance after dose to performance before dose

Subj.	Control Average	M.V.	1 gr.	2 gr.	3 gr.	4 gr.	6 gr.	Caffein Average	Dif. Per Cent.
3	1.008	.040	1.075	1.030	1.010	.930	.913		
			1.027	.900	.930	.940	.881	.963	—.045
			1.051	.965	.970	.935	.897		
			.716	.833	.930	.852	.897		
9	.934	.070	1.002	.947	.895	.970	.854	.889	—.045
			.859	.890	.913	.911	.874		
			1.038	.852	.915	.895	1.025		
14	1.018	.060	.978	.915	.919	.987	.926	.945	—.073
			1.008	.883	.917	.941	.975		
Av.	.986	.056	.972	.912	.933	.929	.915	.932	—.054

son has already been made in the case of Squad II. In tables X–XII. similar figures are given for the other squads, the data for each subject being given separately, along with the squad averages. In these

tables the average of the records before the dose is taken as the norm for the day, and with this norm is compared the average of the records after the dose, the figure given being the ratio of the latter to the former. The various types of days have been kept separate, so that the result of any dose may be compared directly with the average ratios for control days. The difference between these control ratios and the caffein ratios are also given in the final column of the tables.

TABLE XII

TAPPING. SQUAD IV., EXPERIMENT *A*

Ratios of performance after dose to performance before dose

Subj.	Control Average	M.V.	1 gr.	2 gr.	3 gr.	4 gr.	6 gr.	Caffein Average	Diff. Per Cent.
			1.025	1.040	.903	.893	.870		
5	.975	.027	.980	.943	.926	.950	.990		
			1.002	.991	.915	.921	.930	.952	—.023
			.965	1.000	.960	1.010	.903		
6	.978	.051	.970	.943	.980	.950	1.045		
			.968	.971	.970	.980	.974	.972	—.006
			1.017	.990	.905	1.090	1.000		
10	1.023	.032	1.030	.893	.960	.923	.934		
			1.023	.941	.937	1.006	.967	.975	—.048
			.925	.995	.970	.967	.910		
11	.985	.033	1.025	.933	1.088	.967	.990		
			.975	.964	1.029	.967	.950	.977	—.008
			.983	.984	.997	.945	.967		
16	1.013	.038	.986	.955	.947	.935	.965		
			.984	.969	.972	.940	.966	.966	—.047
Av.	.995	.036	.990	.967	.965	.963	.957	.968	—.027

Table X. gives the ratios for Squad I. for seven control days and the same number of pseudo-caffein days, with their M.V.'s. Subjects 1 and 7 are somewhat better on pseudo-caffein days, Subject 4 is better on control days, while Subject 15 shows no difference. The squad averages show a slight superiority on pseudo-caffein days. On the whole then the two types of days balance.

Table XI. gives the records for Squad III. All three subjects show from 4.5 per cent. to 7.3 per cent. stimulation on caffein days, the average being 5.4 per cent. stimulation for all caffein doses. The least stimulation comes from the 1 gr. dose, and the larger amounts do not differ consistently from each other. The previous conclusions concerning this squad are thus completely confirmed.

Table XII., for Squad IV., most strikingly confirms the previous conclusions concerning the effect of caffein on this squad. Without exception the 5 subjects show stimulation on caffein days, ranging in amount from .6 per cent. to 4.8 per cent. The average for the squad, for all doses, is 2.7 per cent. The averages for the various doses show the stimulation to begin with the 1 gr. dose and to increase uniformly with the size of the dose.

In the examination of the effect of caffein on the other tests it will be pointed out that the magnitude of the effect varies inversely with the body weight of the individual. The tapping test is the only exception to this rule. In this case the heaviest individuals (subjects 3, 9, 10, 14 and 16) are more stimulated than are the slighter subjects (5, 6 and 11). The tapping test is again an exception in that the squad taking the dose at the lunch hour yields the highest per cent. of stimulation. In the case of the other tests the greatest effect is shown by the squad taking the caffein in the mid-afternoon, unaccompanied by food.

Experiment *B*

The obvious defect of Experiment *A* is that the action of the dose is not tested at close range nor followed closely for a long period of time, since only five tests a day were made. The rate of action of the drug can not be made out, nor the time of persistence of the effect, nor can its secondary results be accurately determined. As evidence bearing on these points the results of the three-day intensive experi-

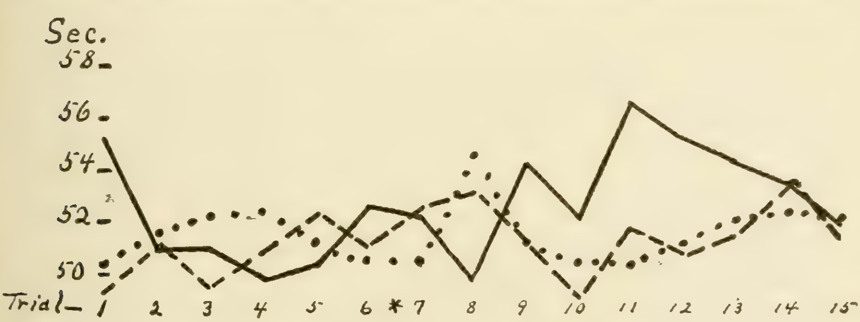


FIG. 4. TAPPING TEST. Experiment *B*. Squad I.

ment are here presented in the form of curves, the unit again being the total time required for the squad to complete the test. In these curves the solid line is the record for March 3, the broken line for March 4, and the dotted line for March 5. The star indicates the time of administering the dose.

Squad I. was the control. On March 3 and 4 only sugar doses

were given. The curves for these two days begin at the same point and show clear tendency to fatigue as the day goes on, dropping slightly in the latter part of the afternoon. This may be considered the normal tendency in the tapping test. On the last day (March 5) this squad was given 3 gr. capsules of caffein after the 6th trial and 2 more gr. an hour and a half after the evening meal. At the 8th test, 1.5 hours after the 3 gr. dose, this third curve rises abruptly, suggesting an initial retardation. The curve then drops to its original level and does not show the fatigue that is clearly present in the two normal curves. The afternoon and evening records are as good as or even better than those of the morning hours before the dose was taken.

Fig. 5. Tapping Test. Experiment *B*. Squad II.

Squad II., on the first day, took 1.2 gr. of caffein (dissolved in a glass of soda fountain syrup and carbonated water) after the 6th trial. At the 8th trial, 1.5 hr. later, the curve drops several seconds and remains low for about 2 hours, after which it rises to the normal fatigue level. On the next day the curve starts out at about the same level as before. Only a sugar capsule was given on this day, and the normal fatigue curve results, the afternoon's record being greatly inferior to that of the day before. On the last morning the curve runs on a high level. After the 6th test 2.4 gr. of caffein were given in the same way as on the first day. The curve at once descends to the lowest level of the first day, showing a gain of nearly 10 seconds. After about 2 hr. it rises again to the normal fatigue level.

Squad III. took 3 gr. of caffein in a capsule, after the 6th trial on the first day. Forty-five minutes later the curve rises abruptly, falling to the original level in 1.5 hr., after which it follows a fairly uniform level with no evidence of fatigue. On the following day the

curve starts at about the original level, rises slightly as the day goes on and then strikes the morning level again. Only sugar was given on this day. There is no evidence of any secondary result of the first day's caffein. The last day was still a control day. There is here only the normal slight tendency to fatigue in the latter part of

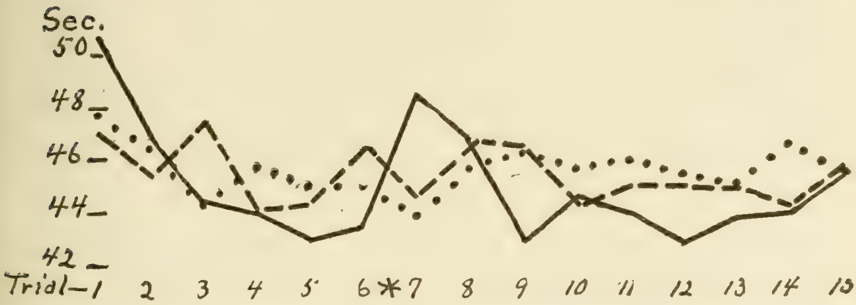


FIG. 6. TAPPING TEST. Experiment *B*. Squad III.

the day, with no evidence of either benefit or disaster as a consequence of the caffein of the first day. The afternoon and evening records on the caffein day remain unequalled by the performance of either of the succeeding days, but the result of the dose seems to have been first a retardation, then counteraction of the tendency to fatigue in the afternoon.

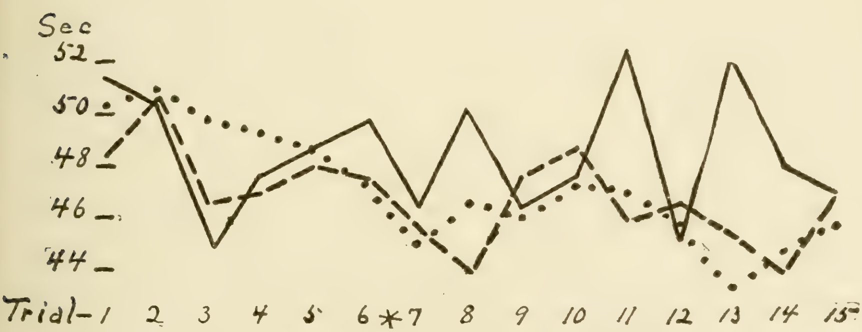


FIG. 7. TAPPING TEST. Experiment *B*. Squad IV.

Squad IV. were given 6 gr. capsules of caffein on the first day and sugar capsules on the days following. The effect of this large dose, taken as it was after several successive control days, seems to have been to produce a great irregularity of performance, six of the subsequent trials breaking all but one of the morning records and

three of them being among the poorest records of the day. The general tendency is toward stimulation, but this stimulation is mixed with an irregularity of performance that resembles the first effect of retardation observed in Squads II. and III. The effect begins as in the other cases, about three quarters of an hour after the dose, and in this case persists about 4.5 hours. On the following day the morning curve behaves much as that of the first day, there being no indication of secondary effect from the first day's dose. As the day goes on the curve is much more regular than that of the preceding day and follows an intermediate course with respect to level. Much the same thing is true of the third day's work.

Experiment *C*

In this experiment, which lasted 7 days, 12 of the subjects comprised a single squad, all receiving the same doses, fifteen minutes before the beginning of each afternoon test. On two days no dose at all was given; on two days glasses of soda fountain syrup containing no caffein; on three days the same syrup with caffein in solution. (For full particulars see Chapter 2.) The results of this experiment, for the tapping test are given in Table XIII. In this table, instead of platting efficiency curves for the different hours and

TABLE XIII

Tapping. Experiment *C*. 15 Subjects

Type of Day		Av. 3 Trials Before Dose	M.V.	Av. 2 Trials After Dose	M.V.	Ratio of Last 2 to First 3	M.V.
Blank	Feb. 6	64.1	7.1	66.3	8.0	1.034	.057
days	11	64.0	8.9	65.5	9.1	1.023	.044
				Av. ratio, blank days ...		1.029	.050
Plain	7	62.8	10.0	61.6	9.5	.971	.049
syrup	9	63.4	7.9	64.1	8.9	1.011	.040
				Av. ratio, syrup days ...		.991	.044
Caffein							
1.2 gr.	8	62.9	7.2	61.2	8.8	.973	.045
3.6 gr.	10	60.3	10.4	61.8	8.9	1.025	.037
6.0 gr.	12	65.1	8.5	60.7	7.9	.932	.044
				Av. ratio, caffein days ..		.976	.042

conditions, the records of the first three periods of each day are averaged to secure the individual's normal efficiency for that day, and with this normal is compared the average of the last two trials of the same day, these latter records having been made after the administration of the dose. The table gives the averages and their

M.V.'s, and the ratios and their M.V.'s, with final averages for each type of day, for the 12 subjects treated as a single squad. Thus 102.9 means that the time of the average performance after the dose was 102.9 per cent. of that of the average performance before the dose. When this per cent. is greater than 100 it indicates fatigue, while a per cent. less than 100 indicates stimulation. Comparison of any two per cents., whether above or below 100 will indicate relative fatigue or relative stimulation. Each final average in the table is the average of 120 trials in the cases of the control days and of the plain syrup days, and of 180 trials in the case of the caffein days.

Referring now to this table, there is seen to be 2.9 per cent. fatigue on control days, .9 per cent. absolute stimulation on syrup days, and 2.4 per cent. absolute stimulation on caffein days. Subtracting the .9 per cent. stimulation yielded by the syrup and carbonated water alone gives a net caffein stimulation of 1.5 per cent. Comparison of these results with the 2.9 per cent. fatigue present on control days indicates a relative stimulation of 3.8 per cent. from the syrup alone, and a relative stimulation of 5.3 per cent. from the syrups with caffein, or a net caffein stimulation of 4.4 per cent.

Summary

Summarizing the results of the three experiments we find:

1. That the typical caffein effect on a motor process such as that involved in the tapping test seems to be a stimulation, which is sometimes preceded by a brief and slight initial retardation.

2. The magnitude of this stimulation (*a*) varies directly with the size of the dose, and (*b*) is relatively slight when the caffein is taken in the forenoon.

3. The effect begins in from 45 to 90 minutes after the administration of the dose, the period being shorter for large doses and longer when the dose is taken along with food.

4. The effect persists for from one to two hours for doses of 1 to 3 gr. and as long as 4.5 hours for 6 gr.

5. There is no secondary or after effect shown within the 72 hrs. over which the intensive doses were traced.

CHAPTER V

The Influence of Caffein on the Steadiness Test

The steadiness test, although not without a certain psychological significance in some connections, is exceedingly difficult to conduct in a satisfactorily rigorous manner. The performance itself, on the part of the individual tested, is easily influenced by factors foreign to the one over which the experimenter may be exercising control. The excitement of taking any kind of dose, or of having accidentally begun in a bungling manner, laughing, a coughing spell, changes in respiration, noises from the street or from adjoining apartments, conspire to produce irregularity and unsteadiness which bear no relation to the influence of caffein. If the hole in which the stylus is inserted is made small enough to betray slight tremors the subject's poise is disturbed by the first few contacts. This was especially true in the present experiment, since the electric counter which registered the contacts was in the room with the rest of the apparatus and every click of the magnet was audible. The use of a counter is in itself most unsatisfactory since faint contacts may fail to actuate the magnet. A longer rod should perhaps be used in order to magnify the amplitude of these small movements. Further, the apparatus described registers at most only movements in the horizontal plane. But it was quite out of the question to employ a more elaborate procedure in the present experiment, and the rather crude method used was adopted in the hope that it might at least afford suggestions bearing on a problem which must otherwise have been entirely ignored.

Experiment *A*

The results for the different squads in Experiment *A* are given in the following tables, in which the records for the various hours on control days have been averaged and may be compared with similar averages of the records after the several amounts of caffein. A separate column indicates the hour and character of the dose. The M.V.'s of all these averages are large, usually about 50 per cent. of the averages themselves. The M.V.'s for Squad I. are given as typical. The variabilities of the other squads are not indicated, since the differences between the average records are only in a few cases large enough to suggest caffein influence.

TABLE XIV

STEADINESS. SQUAD I. EXPERIMENT *A*

Average number of contacts in one minute

Hour		7:45	10:00	12:00	Dose 1:00	3:10	5:30	Cases
Control days	Av.	2.45	2.31	3.66	Sugar	3.89	4.82	10
	M.V.	1.61	1.17	2.94		2.07	2.34	
Pseudo-caffein days	Av.	2.80	2.78	1.88	Sugar	3.00	4.16	10
	M.V.	2.00	1.92	.96		1.33	1.54	

TABLE XV

STEADINESS. SQUAD II. EXPERIMENT *A*

Hour	7:45	10:00	Dose 10:30	12:00	3:10	5:30	Cases
Av.	2.62	1.13	Sugar	1.57	1.50	1.32	10
Av.	2.21	1.55	1–2 gr.	1.43	2.58	3.00	6
Av.	1.50	0.30	4–6 gr.	0.80	2.90	1.10	4

TABLE XVI

STEADINESS. SQUAD III. EXPERIMENT *A*

Hour	7:45	10:00	12:00	Dose 1:00	3:10	5:30	Cases
Av.	1.74	1.83	1.16	Sugar	1.47	1.34	10
Av.	3.17	1.82	2.35	1–2 gr.	1.70	2.07	4
Av.	1.70	1.17	0.50	3–4 gr.	1.50	1.10	4
Av.	0.50	0.40	0.60	6 gr.	0.70	1.80	2

TABLE XVII

STEADINESS. SQUAD IV. EXPERIMENT *A*

Hour	7:45	10:00	12:00	Dose 1:45	3:10	5:30	Cases
Av.	2.64	2.58	3.68	Sugar	2.65	2.98	10
Av.	1.85	1.90	2.75	1–2 gr.	2.00	4.60	4
Av.	2.70	2.75	3.45	3–4 gr.	2.75	3.30	4
Av.	0.60	1.30	1.70	6 gr.	4.30	12.10	2

Table XIV. gives the records for the control squad. There is a uniform tendency on both sets of days for the averages to increase in magnitude at the 3:10 and 5:30 periods. This is apparently a normal fatigue effect, since it is greatest at 5:30 and only slightly present at 3:10. Squad II., Table XV., who took caffein in the mid-forenoon on successive days, shows no similar tendency on control days. Even on the caffein days there is only a slight tendency, which is more marked after small doses than after large. Squad III. (dose with lunch) make much steadier records in the afternoons of control days than in the forenoons. After 1–4 gr. doses this tendency is still suggested by the averages, with no loss of steadiness after the doses.

Only on the days of the 6 gr. dose is the afternoon record inferior to that of the forenoon. On these days there is surprising steadiness (the average being only about 0.5 contacts in a minute) until the 5:30 test, 4–5 hours after the administration of the dose, when the average rises to 1.8. Squad IV., taking doses on an empty stomach at 1:45 P.M., shows uniform performance throughout the day after control capsules. After 1–4 gr. doses of caffein there is a suggestion of unsteadiness at the 5:30 period. After 6 gr. there is clear impairment even at the 3:10 test, the records rising from an average of 1.2 contacts in the forenoon to 4.3 at 3:10 and to 12.1 at 5:30. The most striking effect, as in the case of Squad III., is several hours after the dose.

Experiment *C*

In Experiment *C*, the results of which are to be found in Table XVIII., an attempt was made to secure closer measures by using a smaller hole on the steadiness board (4.5 mm. instead of 6 mm. as in Experiment *A*). Each result given in the table is the average of 12 records each hour of each day, since the whole group of 12 subjects here comprised a single squad. Blank days (no dose) and plain syrup days both show uniform performance at the successive trials. After 1.2–3.6 gr. of caffein the 5:30 record is slightly inferior. After the 6 gr. dose, however, the effect is enormous, the record falling from a morning average of 9.7 contacts to 18.4 at 3:10 and 28.0 at 5:30.

TABLE XVIII

STEADINESS. EXPERIMENT *C*

Doses just preceding 3:10 and 5:30 tests

Hour	7:45	10:00	12:00	3:10	5:30
2 blank days .... Av.	11.0	10.6	10.5	12.4	11.4
2 syrup days .... Av.	13.0	10.5	9.5	11.5	10.5
2 caffein days, 1.2–3.6 gr., with syrup ..	12.2	11.2	11.5	10.6	13.0
1 caffein day, 6 gr. with syrup	8.5	11.1	9.4	18.4	28.0

Experiment *B*

The results of the three-day intensive Experiment *B*, in which the small hole was also used, are given in Tables IX.–XXI. Squad I. shows no great variation during the 15 successive trials on each of the control days. But after the 3 gr. dose of caffein, taken after the 6th trial on March 5th, an increase in contacts of 100 per cent. comes at once, falling off then until the 2 gr. dose was given at a later hour,

TABLE XIX

STEADINESS. EXPERIMENT *B*. MARCH 3

Giving the average for each squad, at each of the 15 trials

Trials	1	2	3	4	5	6	Dose	7	8	9	10	11	12	13	14	15
Squad I.	26.7	22.0	21.3	19.3	11.0	25.3	Sugar	19.0	27.7	14.0	23.7	16.7	16.0	22.6	37.6	22.3
II.	23.5	7.5	26.0	17.0	19.0	15.0	1.2 gr.	12.0	54.0	44.0	28.5	25.5	25.0	19.0	16.5	12.0
III.	16.6	11.8	20.3	10.0	6.2	7.6	3 gr.	9.4	6.8	9.4	13.6	19.0	7.8	9.8	10.2	7.8
IV.	16.7	14.3	16.0	23.0	16.0	19.0	6 gr.	13.3	19.0	12.0	18.0	22.0	22.0	27.3	27.3	11.0

TABLE XX

STEADINESS. EXPERIMENT *B*. MARCH 4

Trials	1	2	3	4	5	6	Dose	7	8	9	10	11	12	13	14	15
Squad I.	31.3	17.0	24.3	11.7	23.0	25.3	Sugar	18.3	25.3	19.0	27.3	24.7	26.7	26.3	15.7	14.0
II.	14.5	10.5	16.5	26.0	27.5	25.0	Sugar	48.5	24.0	15.5	10.5	28.5	26.0	43.5	25.5	19.0
III.	18.0	11.0	8.6	7.8	14.8	8.0	Sugar	12.4	5.6	11.2	8.8	14.8	9.4	5.2	6.0	4.0
IV.	12.7	12.3	10.7	12.0	9.7	10.7	Sugar	6.0	4.3	14.0	7.7	22.0	14.3	8.7	10.0	9.0

TABLE XXI

STEADINESS. EXPERIMENT *B*. MARCH 5

Trials	1	2	3	4	5	6	Dose*	7	8	9	10	11	12	13	14	15
Squad I.	23.0	18.0	20.3	11.3	18.7	34.7	3 gr.	40.0	40.6	35.3	33.3	31.7	23.0	31.7	27.7	14.7
II.	12.0	20.5	13.5	19.5	17.0	8.0	2.4 gr.	3.0	14.5	27.0	11.5	28.5	13.5	21.5	11.5	19.5
III.	5.0	9.2	12.6	9.4	14.4	9.2	Sugar	13.6	9.6	7.8	7.6	16.0	4.6	6.0	4.8	4.6
IV.	30.0	8.3	8.0	13.3	7.6	6.0	Sugar	8.0	6.7	7.0	5.0	14.0	10.3	3.7	7.3	8.0

* Squad I. also took 2 gr. after the 12th trial.

when it rises again. This abrupt effect is quite in contrast to what was found in Experiments *A* and *B*. The discrepancy may perhaps be due to differences in the conditions of the experiment. There was considerable strain involved in the work of these three intensive days, and this caffein day was the third day of that strain. The other tests, however, show no such difference between these days and those of the other experiments. Squad II. shows great irregularity from which no conclusion whatever can be drawn. There is an abrupt rise after each of the small caffein doses but a similar rise is also present immediately after the control dose. We have evidently to deal here with factors foreign to the caffein influence. The behavior of Squad III. after the 3 gr. caffein dose is not different from that after the control doses. Squad IV., after the 6 gr. dose of caffein, shows a marked decrease in steadiness, which does not show itself, however, until about three hours after the dose was taken.

Summary

All three experiments then yield fairly consistent results, in spite of their unsatisfactory technique. After 1–4 gr. of caffein a slight nervousness ensues, which is not apparent until several hours after the dose. After 6 gr. there is pronounced unsteadiness, which begins to be manifested within an hour or so after the dose, but which is still greater after 3–4 hours. Such unsteadiness as is produced is less clearly shown when the caffein is taken in the forenoon or at lunch time than when it is administered in the afternoon, unaccompanied by food. These results are exactly paralleled by the influence of caffein on the quality and quantity of sleep, and suggest an intimate relationship between the measurable tremor produced by caffein on a given muscle group and the evident nervous excitement that is responsible for the insomnia produced by large doses of the same substance.

CHAPTER VI

The Coordination Test

The three motor tests, tapping, steadiness and coordination, required more time for their performance than any other group of tests in charge of a single assistant. In order to avoid delay in the intensive experiment and in the experiment with syrups the coordination test was omitted. This test was chosen for omission for the further reason that in the preliminary working up of the data there seemed to be no indication of any considerable caffein influence. Concerning the time of action, the persistence time, and the presence or absence of secondary effect, therefore, nothing can be stated, in the absence of intensive experiments. As for the value of the test as a means of determining the presence of drug influence, the apparatus in its present form is hardly refined enough to reflect small disturbances of coordination. Skill in performing the test was frequently found to vary for quite unaccountable reasons; often much depends on beginning luckily and striking a favorable rhythm. Further, the measurement of time of performance alone gives no information concerning the precise character of the drug influence—whether it affects speed only, or also actual accuracy, can be known only from observation of the subject's movements on the part of the experimenter. The amounts of the departures from the center of the target are unmeasured, except in so far as they are reflected in the time of recovery which goes to swell the total time of performance.

But in spite of the facts just mentioned, the test does show certain definite suggestions as to the influence of caffein on such coordinations as are involved in performing this three-hole test. These suggestions are further confirmed by the close agreement of the results here obtained, with the results of the experiment on skill in typewriting, a somewhat similar coordination process, though a much more complicated one.

A test involving processes somewhat similar to those used in this coordination test was used by Rivers in his study of the influence of caffein on fatigue. The following quotations are from his report of that experiment.

"The only other research on caffein which I have to record is one carried out in conjunction with Mr. McDougall, using his method of measuring fatigue of attention by estimating the accuracy of aim when dots are made to pass rapidly

through a slit. The work with caffein was done every morning at eight o'clock. The experiment lasted for nine successive days, on three of which a dose of 0.3 gram of citrate of caffein was taken ten minutes before beginning to work. On the other six mornings were taken doses of two control mixtures, which to both of us were quite indistinguishable from that containing the caffein.

"In the first period the number of hits on the caffein days comes out midway between those for the two groups of control days, while in the second period the caffein days are decidedly superior to either group of control days. This superiority of the caffein days is not due to any effect of practise, for the order of the days was varied, and the experiment was only begun after preliminary practise, so that the amounts do not show any definite increase as the result of practise during the nine days that the experiment lasted. The experiment in my own case was carried out on the same days and on exactly the same lines as that of Mr. McDougall, and it agrees with his in showing no indication of a caffein-effect in the first period, but a definite increase in the second."

The following table is a summary of the results of this experiment of Rivers and McDougall.

	McDougall		Rivers	
Number of Hits on	1st Period	2d Period After ½ Hr.	1st Period	2d Period After ½ Hr.
Caffein days	1,576	1,571	1,459	1,460
1st control	1,606	1,424	1,511	1,339
2d control	1,646	1,486	1,602	1,421

It should be noticed, in comparing the results of these experiments with those yielded by the coordination test reported in the present chapter, that Rivers used caffein citrate instead of caffein alkaloid. The dose was 0.3 gramme, which is equivalent to 2.5–3 grains of the alkaloid, in strength. The stimulation which was present in Rivers' experiment will be seen to be present in the coordination test, *but only for the smallest doses* (1–2 grains of caffein alkaloid). When the dose exceeds this amount, the coordination test shows retardation instead of stimulation, as does the typewriting test as well.

Experiment *A*

The results for Experiment *A* are given in the following tables, in which the records are presented in the same way as were those of the tapping test.

Squad I. (control) shows no difference after the dose, between the pseudo-caffein days and the remaining days. The differences balance in magnitude and direction, save that at 5:30 the signs change from + to —, while at 7:45 the +'s predominate. This gives a curious false appearance of retardation on mornings after the pseudo-caffein doses.

Squad II. shows the later periods of the day to be about 3.5 per

TABLE XXII

THREE-HOLE TEST. SQUAD I., EXPERIMENT *A*

	February																		
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
7:45	64.5	65.2	60.6	63.2	59.2	53.7	56.5	56.2	53.0	55.5	55.1	55.0	55.5	54.1	55.7	52.2	50.2	53.9	56.6
10:00	63.6	61.5	55.9	59.7	56.7	54.4	54.9	53.9	54.2	53.7	56.3	58.2	53.2	49.9	51.6	52.9	51.3	52.7	51.2
12:00	61.8	61.9	58.0	58.3	55.8	52.0	52.6	52.6	52.7	66.3	53.2	54.5	55.4	50.2	52.5	46.6	49.1	53.9	52.4
1:00	Dose, sugar doses only, daily.																		
3:10	62.5	61.8	53.3	57.0	57.2	53.2	51.5	50.3	55.0	53.3	53.8	55.7	54.5	52.3	49.9	51.6	54.0	57.0	53.6
5:30	64.0	62.3	60.9	59.5	62.4	55.0	58.9	52.3	53.9	54.5	54.8	53.4	53.6	52.5	50.0	53.9	54.6	56.1	52.5

TABLE XXIII

THREE-HOLE TEST. SQUAD II., EXPERIMENT *A*

	February																			March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	69.5	66.5	*60.3*	*66.2*	*63.1*	57.2	54.5	*55.4*	*52.7*	*60.5*	64.3	56.0	53.7	*52.3*	*52.6*	*51.1*	54.9	52.9	50.9	*52.5*	53.6
10:00	62.8	58.1	*59.8*	*56.8*	*55.8*	51.1	54.7	*48.8*	*53.0*	*40.8*	52.4	54.1	56.1	*52.5*	*52.1*	*48.2*	51.0	52.4	51.0	*49.8*	53.5
10:30	S	S	*1 gr.*	*1 gr.*	*1 gr.*	S	S	*2 gr.*	*2 gr.*	*2 gr.*	S	S	S	*4 gr.*	*4 gr.*	*4 gr.*	S	S	S	*6 gr.*	S
12:00	62.9	61.4	*56.4*	*58.9*	*53.1*	50.1	52.0	*52.0*	*51.1*	*54.0*	52.7	52.8	47.5	*52.1*	*51.2*	*51.1*	53.1	50.9	55.4	*47.4*	53.5
3:10	67.3	59.0	*56.3*	*56.1*	*56.0*	54.9	48.3	*61.0*	*49.1*	*55.4*	55.0	53.5	49.8	*53.7*	*52.5*	*52.3*	48.1	52.7	54.9	*51.7*	50.6
5:30	65.8	62.3	*59.1*	*62.0*	*58.3*	61.8	55.0	*52.0*	*50.9*	*51.4*	56.4	53.1	52.1	*54.0*	*53.2*	*53.7*	54.4	52.7	51.4	*51.4*	54.0

TABLE XXIV

THREE-HOLE TEST. SQUAD III., EXPERIMENT *A*

	February																			March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	57.6	*55.0*	51.3	*51.5*	53.6	*51.7*	50.1	*49.2*	48.0	*54.7*	51.0	*50.3*	53.7	*50.5*	49.9	*50.8*	49.9	*48.7*	49.6	*50.3*	59.3
10:00	52.8	*54.7*	50.7	*49.1*	52.1	*47.2*	48.0	*49.2*	52.4	*50.2*	51.9	*50.3*	53.1	*44.8*	45.4	*52.7*	48.6	*46.5*	47.1	*45.7*	49.3
12:00	53.9	*51.6*	50.4	*54.5*	50.9	*45.7*	48.9	*45.7*	48.1	*48.7*	50.4	*50.1*	50.2	*47.7*	47.5	*49.7*	47.3	*47.4*	50.1	*45.7*	47.0
1:00	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	54.5	*57.0*	50.5	*53.5*	54.2	*49.0*	51.0	*50.2*	51.4	*48.6*	49.1	*54.4*	48.1	*48.0*	48.9	*48.9*	48.2	*50.0*	48.8	*49.6*	50.3
5:30	51.4	*54.7*	57.5	*51.1*	53.7	*49.1*	51.7	*48.7*	49.1	*52.0*	50.7	*48.4*	49.1	*50.1*	49.9	*52.1*	53.3	*48.9*	52.7	*53.1*	50.3

TABLE XXV

THREE-HOLE TEST. SQUAD IV., EXPERIMENT *A*

	February																			March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	64.3	*65.6*	62.7	*61.0*	55.4	*55.7*	55.4	*55.0*	56.0	*53.6*	58.7	*52.4*	56.1	*52.0*	52.6	*52.4*	53.1	*51.7*	57.6	*54.4*	55.5
10:00	64.0	*61.0*	54.3	*57.2*	57.0	*54.7*	56.2	*53.4*	50.9	*53.6*	55.1	*57.2*	53.5	*51.9*	53.4	*50.0*	53.0	*55.0*	52.7	*49.2*	52.9
12:00	63.5	*63.0*	57.4	*55.4*	57.5	*55.7*	53.8	*54.0*	55.6	*54.8*	54.4	*51.5*	54.4	*52.4*	52.3	*52.0*	50.2	*53.4*	52.6	*53.5*	52.2
1:45	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	66.4	*59.0*	59.5	*56.8*	59.0	*54.7*	52.4	*49.0*	52.6	*54.2*	53.4	*51.0*	49.9	*52.0*	49.1	*47.9*	51.8	*53.0*	51.6	*54.2*	52.0
5:30	63.6	*61.8*	60.7	*56.9*	57.6	*56.8*	51.6	*52.0*	51.6	*54.7*	52.8	*52.8*	51.6	*54.6*	52.1	*52.7*	53.3	*55.0*	51.7	*54.4*	52.1

TABLE XXVI

COORDINATION. SQUAD I

7:45 Sug. Av.	7:45 Caf? Av.	Dif.	10:00 Sug. Av.	10:00 Caf? Av.	Dif.	12:00 Sug. Av.	12:00 Caf? Av.	Dif.	1:00 Dose	3:10 Sug. Av.	3:10 Caf? Av.	Dif.	5:30 Sug. Av.	5:30 Caf? Av.	Dif.
61.3	64.2	— 2.9	58.1	60.6	— 2.5	58.4	60.1	— 1.7	S	56.6	59.4	— 2.8	62.1	60.9	+ 1.2
56.4	55.0	+ 1.4	55.2	54.2	+ 1.0	54.0	52.3	+ 1.7	S	53.9	51.8	+ 2.1	58.5	53.7	+ 4.8
59.7	55.2	+ 4.5	55.1	56.0	— 0.9	53.7	55.4	— 1.7	S	53.8	54.5	— 0.7	54.2	54.0	+ 0.2
54.3	53.2	+ 1.1	52.0	54.6	— 2.6	55.9	48.4	+ 7.5	S	56.3	52.0	+ 4.3	52.0	53.2	— 1.2
54.6	52.8	+ 1.8	51.3	51.8	— 0.5	52.0	51.7	+ 0.3	S	53.9	55.4	— 1.5	53.4	55.6	— 1.2

TABLE XXVII

COORDINATION. SQUAD II

Dose	Average 1st and 2d Normal	Third Trial Per Cent. of Normal	Fourth Trial Per Cent. of Normal	Fifth Trial Per Cent. of Normal	Average 3d, 4th and 5th Per Cent. of Normal	Cases
Sugar Av.	57.2	94.9	94.9	99.8	96.5	11
Caffein 1 gr.	60.2	93.0	93.0	99.1	95.0	3
Caffein 2 gr.	51.8	101.1	106.5	99.2	102.3	3
Caffein 4 gr.	52.0	99.0	101.0	103.0	101.0	3
Caffein 6 gr.	51.2	92.5	100.9	100.3	97.9	1
Caffein Av.	53.8	96.4	100.4	100.4	99.1	10

TABLE XXVIII

COORDINATION. SQUAD III

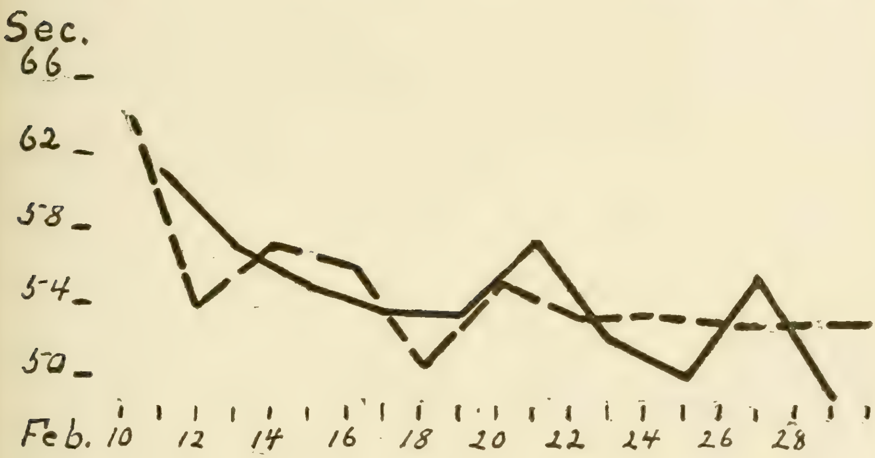
7:45			10:00			12:00			1:00	3:10			5:30		
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
53.5	53.3	+ 0.2	51.4	51.9	— 0.5	51.5	53.1	— 1.6	1 gr.	52.5	55.3	— 2.8	55.3	52.9	+ 2.4
50.5	50.5	0.0	49.9	48.8	+ 1.1	49.2	45.7	+ 3.5	2 gr.	51.9	49.6	+ 2.3	51.6	48.9	+ 2.7
50.8	52.5	— 1.7	52.4	50.3	+ 2.1	49.8	49.4	+ 0.4	3 gr.	49.5	51.5	— 2.0	47.4	50.2	— 2.8
50.7	50.7	0.0	48.2	48.8	— 0.6	48.2	48.7	— 0.5	4 gr.	48.6	48.5	+ 0.1	50.6	51.1	— 0.5
52.1	49.5	+ 2.6	48.0	46.1	+ 1.9	48.7	46.6	+ 2.1	6 gr.	49.6	49.8	— 0.2	52.2	51.0	+ 1.2

TABLE XXIX

COORDINATION. SQUAD IV

7:45			10:00			12:00			1:45	3:10			5:30		
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
61.3	63.3	— 2.0	57.6	59.1	— 1.5	59.0	59.2	— 0.2	1 gr.	61.1	57.9	+ 3.2	60.7	59.4	+ 1.3
55.6	55.4	+ 0.2	55.2	54.1	+ 0.9	55.2	54.9	+ 0.3	2 gr.	54.1	51.9	+ 2.2	53.1	54.4	— 1.3
57.4	53.0	+ 4.4	53.6	55.4	— 1.8	54.7	53.7	+ 1.0	3 gr.	52.4	52.6	— 0.2	52.2	53.8	— 1.6
53.6	52.2	+ 1.4	53.4	51.0	+ 2.4	52.4	52.2	+ 0.2	4 gr.	50.0	45.0	+ 5.0	52.3	53.7	— 1.4
54.9	53.1	+ 2.8	52.8	52.1	+ 0.7	51.9	53.3	— 1.4	6 gr.	51.8	53.6	— 1.8	52.2	54.7	— 2.5

cent. superior to the normal for the control days, while the average of all the caffein days is about the same at the first trial after the dose, but becomes .4 per cent. inferior as the day goes on, an average retardation of 4 per cent. as compared with control days. But the various sized doses do not produce quite the same effect. The 1 gr. dose seems to yield slight stimulation at all later trials, from 1.9 per cent. at the 3d and 4th trials to .7 per cent. at the last. The 2 gr. dose, however, produces immediate inferiority over control days of 6.2 per cent. at the 3d, and 11.6 per cent. at the 4th, the retardation having disappeared at the 5th trial. The 4 grains produce retardation for all three trials, as compared with control days, and for both



Coordination Test. Experiment *A*. Squad IV

Broken line represents control days and solid line caffein days. Dose taken in mid-afternoon, on empty stomach.

Fig. 8. 10:00 a.m. No difference shown.

the 2 gr. and 4 gr. amounts the average performance after the dose is inferior to the normal instead of superior, as on control days. The 6 gr. dose produces at the next trial, the best record in the table, a superiority of 7.5 per cent. over the normal and of 2.4 per cent. over the corresponding record for control days. At the following trials, however, there is both absolute and relative retardation. The average for all caffein doses shows, at the successive trials after the dose, an inferiority of 1.5 per cent., 5.5 per cent. and .5 per cent. as compared with the corresponding records for control days. Generally speaking, for this squad small doses stimulate for the rest of the day; medium doses retard for the next two trials, while the large dose first stimulates and then retards.

Squad III. shows balanced records throughout, with the possible exception that at 5:30 there seems to be an effect similar to that noted with Squad II. Small doses (1–2 gr.) stimulate, medium (3–4) retard, while 6 gr. yields evidence of slight initial stimula-

FIG. 9. 12:00 M. No difference shown.

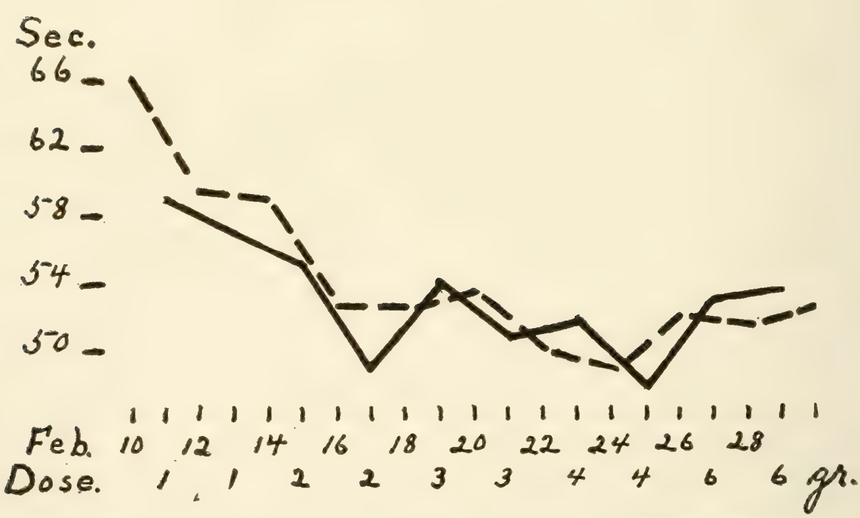


FIG. 10. 3:10 P.M. About one hour after dose. No difference.

tion (remembering that the drug action is doubtless retarded here by the fact that the dose was taken at lunch time, along with food substance).

Squad IV., at 3:10, shows stimulation for 1–4 gr. At 5:30 this effect persists for 1 gr. but all other amounts show retardation. For 6 gr. this retardation was already present at 3:10. At 10:00 and 12:00 the differences balance, but at 7:45 there is the same appearance of retardation that was noted in the case of Squad I., the control squad. Were it not for this latter fact one would be inclined to take the figures as evidence of continued retardation on the mornings after caffein days. Perhaps, indeed, the conclusion that small amounts of caffein stimulate should be pronounced with some caution, for a similar relation of the figures occurs at the 5:30 test for Squad I. as a result of sugar doses only. But that this again is a false appearance is indicated by the fact that at the preceding trial, 2 hours after the dose, no such relation is present, with this control squad, while in Squads II., III. and IV. the two trials (3:10 and 5:30) are consistent. The following curves show for Squad IV. the

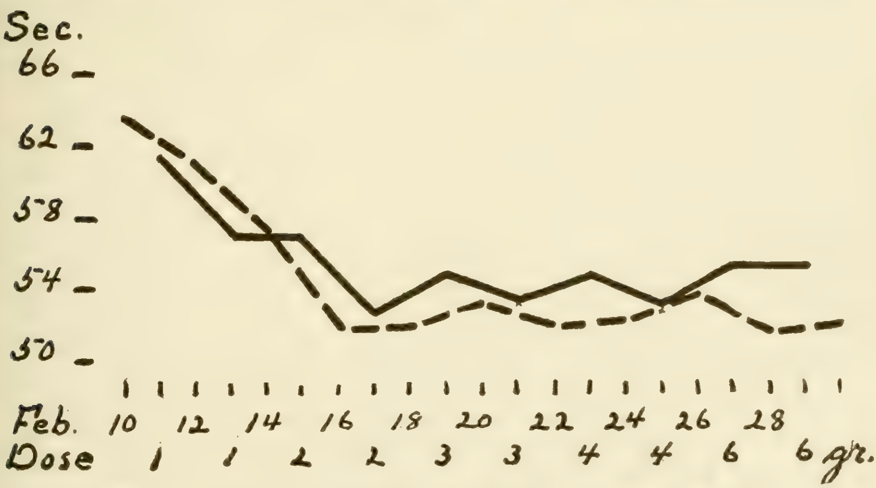


FIG. 11. 5:30 P.M. Evidence of retardation on caffein days, for doses larger than 1 grain.

records for the two trials before and the two after the doses, on each of the 21 days. As in the case of the tapping test, the broken line represents the records for control days and the solid line those for caffein days. The curves for the 10:00 and 12:00 periods show no difference between control and caffein days. But at 3:10 the stimulation from 1–2 gr. is indicated, while at 5:30 there is uniform retardation for amounts larger than 1 gr.

Another way of treating these data is to compare the performance after the caffein dose with the performance of the earlier trials of the same day, instead of with the records for corresponding hours on

control days. Thus if the average of the trials before the dose be considered the standard for the day, the ratio of the trials after the dose to this standard, will give, when ratios for control days and for caffein days are compared, a measure of the caffein influence if such is present. The results from the Three Hole test, when thus treated, are given in Tables XXX.–XXXIV. The ratios for Squad I. (Table

TABLE XXX

Three-hole Test. Squad I., Experiment *A*

Showing ratio of performance after dose to performance before dose

Subj.	Control Days Av.	M.V.	Pseudo-caf. Days Av.	M.V.
1	1.010	.040	1.010	.034
4	.992	.048	.991	.070
7	.973	.029	.995	.040
15	.989	.045	.985	.050
Average	.991	.040	.995	.048

TABLE XXXI

Three-hole Test. Squad II., Experiment *A*

Showing the ratio of the 12:00 and 3:10 records (Av.) to the performance before the dose at 10:30 A.M.

			Caffein								
Subj.	Control Average	M.V.	1 Gr.	M.V.	2 Gr.	M.V.	4 Gr.	M.V.	6 Gr.	Av. of 1 and 2 Gr.	Av. of 4 and 6 Gr.
8	.977	.052	.959	.050	.919	.030	.990	.040	1.024	.939	1.007
12	.986	.049	.941	.040	.991	.023	1.017	.040	.910	.966	.964
13	.936	.046	.953	.016	.943	.020	1.026	.016	.920	.948	.973
Av.	.969	.049	.951	.035	.951	.024	1.011	.032	.951	.951	.981
Difference between Control Av. and final Caffein Av.										—.018	+.012

TABLE XXXII

Three-hole Test. Squad II., Experiment *A*

Showing ratio of 5:30 test to performance before dose at 10:30 A.M.

			Caffein								
Subj.	Control Average	M.V.	1 Gr.	M.V.	2 Gr.	M.V.	4 Gr.	M.V.	6 Gr.	Av. of 1 and 2 Gr.	Av. of 4 and 6 Gr.
8	1.047	.063	1.035	.053	.960	.043	.998	.046	1.041	.997	1.019
12	.951	.045	.928	.026	1.020	.043	1.053	.073	.994	.974	1.023
13	1.028	.079	1.079	.020	.991	.073	1.076	.043	.978	1.035	1.027
Av.	1.008	.062	1.014	.033	.990	.053	1.042	.054	1.004	1.002	1.023
Difference between Control Av. and Caffein Av.										—.006	+.015

XXX.) on caffein and control days, do not differ. This holds for all four subjects as well as for the final average for the squad.

In the case of Squad II., the results for the first two trials after the dose have been computed in one table, and the results of the

TABLE XXXIII

THREE-HOLE TEST. SQUAD III., EXPERIMENT *A*

Showing ratio of records after dose (Av. of 3:10 and 5:30 trials) to record before dose (Av. of 8:00, 10:00 and 12:00 trials)

Subj.	Control Average	M.V.	1 Gr.	2 Gr.	Caffein 3 Gr.	4 Gr.	5 Gr.	Average 1 and 2 Gr. (Also Difference Between These and Control Average)	Average 4 and 6 Gr.
3	1.009	.040	.931	.983	1.011	1.058	.976	.956	1.004
			.955	.950	1.054	.968	1.027	—.043	—.005
			.943	.970	1.032	1.008	1.001		
9	1.042	.041	1.028	1.032	1.012	1.034	1.042	1.004	1.044
			1.043	.912	1.014	1.098	1.002	—.038	+.002
			1.036	.972	1.013	1.066	1.022		
14	1.005	.064	1.068	.972	.959	.936	1.043	1.065	1.049
			1.055	1.168	.996	.987	1.230	+.060	+.044
			1.061	1.070	.978	.961	1.137		
Av.	1.019	.048	1.013	1.004	1.008	1.012	1.053	1.008	1.033
								—.011	+.014

TABLE XXXIV

THREE-HOLE TEST. SQUAD IV., EXPERIMENT *A*

Ratio of records after dose to records before dose

Subj.	Control Average	M.V.	1 Gr.	2 Gr.	Caffein 3 Gr.	4 Gr.	6 Gr.	Average 1 and 2 Gr. (Also Difference Between These and Control Average)	Average 4 and 6 Gr.
5	.976	.089	.886	1.085	1.040	1.011	1.067	.999	1.058
			1.080	.945	1.027	1.084	1.072	+.023	+.082
			.983	1.015	1.033	1.047	1.069		
6	1.009	.029	.935	.985	1.018	1.087	1.045	.965	1.035
			1.008	.933	1.022	.942	1.065	—.034	+.026
			.971	.959	1.020	1.015	1.055		
10	.978	.028	1.001	.995	.968	1.058	1.015	.975	1.035
			.957	.956	1.041	1.035	1.043	—.003	+.057
			.979	.971	1.004	1.041	1.029		
11	.999	.093	.908	.895	1.078	.984	.884	.893	.952
			.925	.848	.874	.907	1.035	—.106	—.047
			.916	.871	.976	.945	.959		
16	.970	.050	.935	1.117	.914	.977	1.100	.961	.992
			.900	.896	.816	.932	.960	—.009	+.022
			.917	1.006	.865	.954	1.030		
Av.	.987	.057	.953	.964	.979	1.000	1.028	.958	1.014
								—.025	+.027

5:30 trial in another. In addition to the ratios and their M.V.'s the difference between the final averages for control and for caffein days is given, the 1 and 2 gr., and the 4 and 6 gr. doses being here averaged together. The results from both these tables confirm the statements already made concerning this squad—the small doses result in stimulation while the large doses are followed by retardation. The effect on this squad is, however, slight, and the three individuals do not all closely follow the rule drawn from the squad average. The small amount of both stimulation and retardation found here is apparently due to the time of day at which the dose was taken (10:30 A.M., a point of maximum efficiency).

In the case of Squad III. the separate records for each of the caffein days are given, as well as the averages. Subject 3 is always stimulated by the 1 and 2 gr. amounts. For larger amounts the majority of the days show retardation, only two days showing stimulation. Averaging the 1 and 2 gr. gives 4.3 per cent. stimulation, while the average of the 3, 4 and 6 gr. amounts gives retardation. The same is true of subject 9, while subject 14 is retarded by both small and large doses. The average for the squad gives 1.1 per cent. stimulation for small and 1.4 per cent. retardation for large doses. These amounts are somewhat larger than those of Squad II.

With Squad IV., the result is most striking and the magnitude of the effect greatest. The average of the five subjects gives 2.5 per cent. stimulation for small doses and 2.7 per cent. retardation for large. All the subjects follow much the same rule, and it is apparent that the magnitude of the caffein effect varies with body weight. Subjects 5 (105 lbs.) and 11 (110 lbs.) are affected by as much as 8 and 10 per cent. Subjects 6 (125 lbs.) and 10 (157 lbs.) run only as high as 3 to 6 per cent., while the heaviest subject, No. 16 (174 lbs.) has a maximum average of only about 2 per cent.

Thus whether the corresponding records of control days, or the average performance on a given day before the dose is taken, be adopted as the standard of comparison, the same result is indicated. In all respects these results resemble closely those yielded by the typewriting test, which is reported in another chapter. In the typewriting test the speed of performance is quickened by small doses of caffein (1 to 3 gr.) and retarded by larger amounts (4 to 6 gr.).

Summary

The following statements are borne out by the data. The effect of small amounts of caffein on the coordination test is stimulation, while that of larger amounts is retardation. There is some evidence

that the retardation produced by the largest amount (6 gr.) is subsequent to a slight initial stimulation. The caffein effect is slight in the case of the squad taking the dose in the A.M. It is somewhat greater in the case of the squad taking the dose at the lunch hour, and is greatest of all when the dose is taken in the P.M. without food substance. Further, the magnitude of the caffein effect varies inversely with the body weight of the individual tested.

CHAPTER VII

Influence of Caffein on Speed and Quality of Performance in Typewriting[1]

The greater part of the previous work on the influence of drugs has been directed toward the study of relatively simple mental and motor processes such as reaction times, free and controlled associations, reading, adding, hitting at dots, and especially the production of ergograms. In the present experiments, in addition to the investigation of the series of similarly simple processes reported in the preceding chapters, an attempt was made to measure the influence of caffein alkaloid on a more complicated process, that of performance in typewriting.

Subject No. 2, a woman of 38 years, already fairly proficient in typewriting by the touch method, did not take part in the tests through which the previously described squads were put. Instead she made systematic records of her skill in typewriting throughout the four weeks. Ruskin's "Sesame and Lilies" was chosen as the material to be copied, since it was fairly uniform in character and interest throughout, and was unfamiliar to the subject. The pages of the edition used contained 27 lines, the lines containing on the average 35 characters (letters and punctuation marks). The pages were placed in a random order on an improvised holder, directly over the machine and on a level with the writer's eyes. Care was taken to keep the lighting conditions as constant as possible and the amount of disturbance through the day at a minimum. The subject corrected all mistakes noticed at the time they were made, and record was made (1) of the time taken to write the standard amount, (2) the number of corrected errors and (3) the number of errors passing undetected. The time record was kept by the subject herself, but the errors were counted after the close of the experiment, by a second person and checked up by a third.

During the first 27 days of the experiment the standard amount was 3 pages. This amount was written 7 times daily, the hours being at 8:00, 9:00, 10:00, 11:00, 2:00, 3:10 and 5:30, in order to distribute the trials as much as possible over the entire day. During the first week only sugar doses were taken, the object being, as in the case of

[1] This chapter is reprinted from the *Psychological Review*, January, 1912.

the squads already described, to reach a practise level and to secure perfect adaptation to the conditions of the experiment before the caffein was administered. After the first week caffein doses were given, in capsule form, on alternate days, the subject being in no case able to distinguish between the caffein days and the control days. This arrangement gave two days for each of the 1, 2, 3, 4 and 6 grain doses employed. The doses were given in increasing amounts, and in all cases just after the first trial for the day had been made, this time being about 8:30 A.M. When caffein is taken in capsule form its effect does not begin until about one hour after taking. Consequently, besides comparing the absolute amount of work done on caffein days with the amount done on control days, the first two trials of each day may be used as a normal performance for that day and the ratio of the five later trials to this normal computed for both kinds of days.

On the remaining 3 days (the intensive experiment) the subject came to the laboratory daily at 10:00 A.M. and wrote 2 pages each half hour (excepting short intermissions for lunch and dinner) until 9:15 P.M. thus making 19 trials each day. On the first of these three days 3 grains of caffein were taken at 3:15 P.M., just before beginning the 10th trial. On the second day a control capsule was taken at the same hour and on the third day a 6 grain dose of caffein. During these days there was absolutely no evidence of practise effect, the subject having reached her level some time before the intensive experiment began. It should be stated that when the book had been copied through once its pages were shuffled again and re-written in random order.

Rivers has made some use of the typewriting test in his work on the effects of caffein and alcohol by inserting periods of writing between the successive performances on the ergograph. In the case of alcohol neither the speed nor the accuracy of the writing seemed to be affected. "There is certainly no indication of any favorable action of the alcohol" (p. 96). "The errors in typewriting fall into two classes—those which escape notice and those which are noticed and corrected. . . . It will be seen that the latter are not very numerous, and so constant in number that they give not the slightest indication of an alcohol-effect. The uncorrected errors occur more frequently, and show an unmistakable tendency to increase with the rapidity of the work, being most numerous in the second interval of the fifth day, when the amount of work reached its maximum. When this increase with rapidity of work is taken into account, there is no definite indication of any alcohol-effect" (pp. 97–8).

There is however a striking discrepancy between these statements

of Rivers and the table (p. 96) on which he bases them. The table referred to is given complete below and the discrepancy pointed out because of its bearing on certain results of the present experiment.

TABLE III[2]

	May 17 No dose	May 19 Control	May 21 40 c.c.	May 23 20 c.c.	May 25 No dose	May 27 20 c.c.	May 29 Control	May 31 Control
1st interval								
Quantity of work	832	824	841	884	883	847	871	902
Corrected errors	47	56	86	80	89	74	86	94
Uncorrected errors	26	30	38	26	39	27	21	31
2d interval								
Quantity of work	797	842	805	884	956	897	885	904
Corrected errors	86	71	80	92	140	99	107	127
Uncorrected errors	45	46	31	26	42	36	44	19

Contrary to the statements quoted in the preceding paragraph, the *corrected* errors are without exception much more numerous than the *uncorrected.* This, it will later be seen, was also the case in the present experiment. The absolute numerical proportion between the two types of errors is of course immaterial and even their relative numbers would depend chiefly on the attitude of the subject toward the question of corrections.

In River's experiments with caffein .3 gram of caffein citrate, equivalent in strength to about 2.5–3 grains of the alkaloid, was taken morning and evening for 6 days, and on mornings only for another 6 days adequate control doses being employed (gentian and citric acid). The dose was taken 10 minutes before the work began. With respect to speed, this experiment showed "the distinct superiority of the caffein days" (p. 45). The number of mistakes was also determined and "here it came out quite definitely that the drug was without influence."

Of the three most available methods of presenting the results of the present experiment only two show clear results. One might on the one hand map out the efficiency curve for the various days, thus indicating, on each day, the course of the performance before and after taking the dose. But diurnal variations arising from other causes obscure the relatively slight influence of the drug from this point of view. A more satisfactory way is to compare the total amount of work done on the caffein days, after the dose has been taken with the similar records afforded by the control days, thus indicating the general capacity for work rather than the diurnal course of efficiency. Or instead of the total amount of work done we may use to advantage the ratio of this amount to the normal work of the

[2] Rivers, "The Influence of Alcohol and Other Drugs on Fatigue," p. 96.

respective days, the normal consisting of the work done before the dose was taken.

From the point of view of speed of performance both of these latter methods show clearly that doses of 1–3 grains of caffein alkaloid are stimulating in their effect while larger doses (4–6 grains) produce retardation. Fig. 12 records the total time taken for the 6 trials after the capsule was taken, for each of the 19 days of the first series of tests, beginning with the last day before the caffein doses commenced. The broken line follows the records for the control days and the solid line that for the caffein days, the time being recorded in minutes. The 1 and 2 gr. doses show indication of decided stimula-

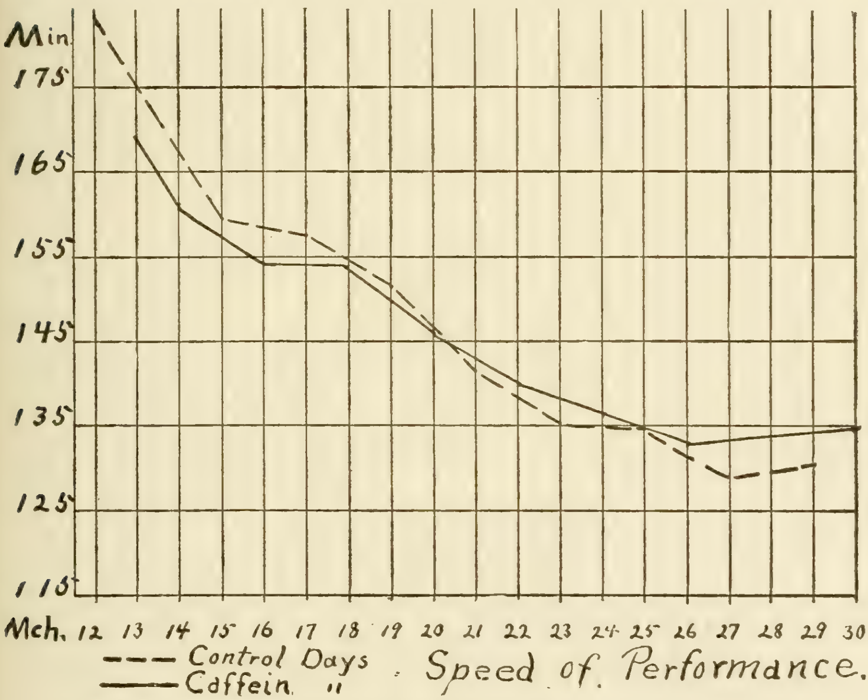


FIG. 12. SPEED IN TYPEWRITING. Showing the total time required for the six trials after the dose, on each day. Broken line represents control days and solid line caffein days.

tion, but at the 3 gr. dose the curves cross, the larger doses of caffein yielding longer times than those of the control days. Presented in this manner, however, the stimulation is somewhat obscured by the practise effect shown by the curves as a whole.

In order to eliminate this factor of practise to a greater degree

and to allow for daily variations of an irrelevant sort, I have computed the ratio of the average performance after the dose to the normal performance of each day, this normal being secured by averaging the first two morning trials. These ratios give the curves of Fig. 13 in which again the broken line represents the ratios for the control days and the solid line those for the caffein days. The effect suggested by the curves of Fig. 12 is here very clearly repeated, except that the retardation does not appear until the 4 grain dose is reached. The curve for the control days is practically a horizontal

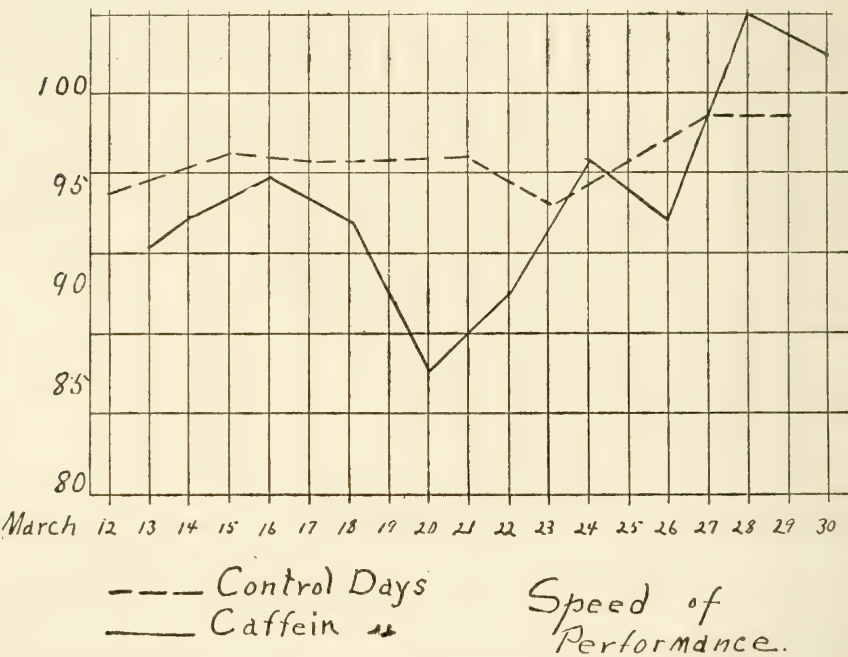


FIG. 13. SPEED IN TYPEWRITING. Showing the ratio of the average performance after the dose to the normal performance before the dose on the corresponding day. Broken line represents control days and solid line caffein days.

line, showing uniform maintenance of the normal throughout the control days. But the solid line shows improvement over the daily normal after doses of 1–3 grains and retardation below the daily normal after larger doses.

Table XXXV. shows the final averages of the daily totals for the last five trials of all the days. The control days and all the caffein days, regardless of the size of the dose, are averaged, and the time, the number of corrected errors, the number of uncorrected errors and the total errors recorded in separate columns.

TABLE XXXV

Averages for	Time	Errors Corrected	M.V.	Errors Uncorrected	M V.	Total Errors
Control days	146.7	208	7	73	14	281
Caffein days	145.7	201	11	66	13	267

The average times balance, on account of the compensating effects of the small and large doses. The averages for both kinds of errors are smaller on caffein days than on control days, but the mean variations are fairly large and the difference is quite within the range of the probable error. Nevertheless the fact that it is a genuine difference is apparently borne out by the results of the intensive experiment which follow. But the difference is so small that curves corresponding to those for the times fail to show anything clearly.

Table XXXVI. gives the results of the three-day intensive experiment, in which 19 trials were made, the dose being taken at the time of the 10th or median trial. The table gives the totals for the 9 trials before the dose, comparing these with the totals for the 9 trials following it.

TABLE XXXVI

INTENSIVE EXPERIMENT

		First Nine Trials	Last Nine Trials	Difference	Per Cent. Loss or Gain
	Control	128.6	132.1	+ 3.5	+.027
Time	3 grains	130.1	129.5	— .6	—.005
	6 grains	131.5	131.3	— .2	—.002
Corrected	Control	189	176	— 13	—.068
errors	3 grains	216	156	— 60	—.277
	6 grains	273	170	—103	—.377
Uncorrected	Control	35	49	+ 14	+.400
errors	3 grains	42	35	— 7	—.167
	6 grains	59	34	— 25	—.423
Total	Control	224	225	+ 1	+.005
errors	3 grains	258	191	— 67	—.259
	6 grains	332	204	—128	—.385

These figures confirm the previously drawn conclusions. With respect to speed of performance the slight fatigue present on control days gives place to very slight stimulation on caffein days. The actual difference in time is, however, so slight as to be, in this case, scarcely worth mentioning. And this is what we should expect when the 3 and 6 grain doses are taken. But the difference in the number of errors of both kinds is very great. On control day the corrected errors are slightly less after the dose than before it (— 6.8 percent.). But after the 3 grain dose they decrease much more decidedly

(— 27.7) and after the 6 grains still more so (— 37.7 per cent.). The uncorrected errors are greater for the last 9 trials on control day, clearly less after 3 grains of caffein (— 16.7) and strikingly less after 6 grains (— 42.3 per cent.). Compared with the first 9 trials the total errors for the last 9 trials are greater for the control day, less for the 3 grain dose, and still less for the maximum dose of caffein. Contrasted with Rivers' result for alcohol, the time and the errors grow less, simultaneously, and the superiority of the work, from the point of view of errors, which did not seem to be present in Rivers' tests of caffein, is clearly shown. But this superiority is seen only when the general capacity for work, over a considerable period of time, is examined. When the results are platted so as to show the course of the performance throughout the day, the curves are obscure.

Summary

The speed of performance in typewriting is quickened by small doses of caffein alkaloid (1–3 grains) and retarded by larger doses (4–6 grains). The quality of the performance, as measured by the number of errors, both corrected and uncorrected is superior, for the whole range of caffein doses (1–6 grains), to the quality yielded by the control days. Both types of errors seem to be influenced to about the same degree. The increase in speed is not gained at the expense of additional errors, but increased speed and decreased number of errors are simultaneously present.

CHAPTER VIII

The Influence of Caffein on the Color-naming Test

When the investigation began, a few preliminary trials seemed to indicate that ordinary daylight would afford conditions which would be so constant as to produce no great variability in the color-naming test as the result of fluctuating illumination. But the work had not progressed far before it was found that the diurnal variations in sunlight were so great as to constitute a disturbing factor in the records. On most days there was insufficient daylight at the 5:30 test for the colors to be promptly distinguished, while the introduction of such artificial light as was then available changed appreciably the color tone of the bits of paper. Rather than disturb the experiment when it was well under way and trusting to the 3:10 trial for indications of the character of the caffein influence, if such were present, it was decided to omit the color-naming test at the 5:30 period in Experiment *A*, thus giving only one trial after the dose. The result was that nothing came of this test in Experiment *A*. Such influence as was present was too tardy to be revealed so soon after the dose, hence the 3:10 records differ in no respect from those of the three trials before the dose. In Experiments *B* and *C* a gas mantle was secured which gave a fairly pure white light and the test was conducted in this artificial illumination at all trials, including 5:30.

Experiment *C*

Experiment *C* will serve to give an introductory view of the character of the caffein influence on this fairly simple association process. Table XXVII. gives the records (Av. and M.V.) for the one squad of 12 subjects used here, for each of the five trials on the various types of days. Further, since the doses were taken before the 3:10 and 5:30 trials (15 minutes before the trials began) the 7:45, 10:00 and 12:00 records have been averaged to constitute a normal record for each type of day and the two trials (3:10 and 5:30) following the dose, when averaged, give the indication of caffein influence. In a final column is given the ratio of this afternoon average to the normal performance in each case.

The blank days (no dose whatever) show fatigue in the latter part of the day, the afternoon's work averaging 105.4 per cent. of

TABLE XXXVII

COLOR-NAMING. EXPERIMENT *C*

Trial		7:45	10:00	12:00	3:10	5.30	Av. 1st 3	Av. 4 and 5	Av. 4 and 5 Per Cent. of Av. 1, 2 and 3
Blank days,	Av.	54.8	53.4	58.4	58.9	58.0	55.5	58.5	105.4
24 cases	M.V.	9.5	7.0	8.6	7.2	7.0	8.4	7.1	
Syrup days,	Av.	56.0	55.8	60.0	55.2	57.3	57.3	56.2	98.1
24 cases	M.V.	8.8	6.0	7.4	9.4	9.1	7.4	9.3	
Syrup and 1.2 gr.	Av.	53.8	57.2	59.2	53.0	55.9	56.8	54.5	95.9
caffein, 12 cases	M.V.	9.4	7.1	8.3	6.2	8.5	8.3	7.3	
Syrup and 3.6 gr.	Av.	55.4	57.3	61.2	54.5	55.5	58.0	55.0	94.8
caffein, 12 cases	M.V.	9.1	8.6	8.8	9.3	9.7	8.5	9.5	
Syrup and 6 gr.	Av.	59.5	56.4	60.4	56.4	58.1	58.8	57.3	97.4
caffein, 12 cases	M.V.	13.4	9.5	9.2	9.5	11.4	10.7	10.5	
Caffein Av.,	Av.	56.2	56.9	60.9	54.6	56.5	57.9	55.6	96.0
36 cases	M.V.	10.6	8.4	8.8	8.3	9.9	9.3	9.1	

the normal. On the plain syrup days the 12:00 o'clock period shows the same tendency beginning, but after the syrup doses (taken with carbonated water) the record falls instead, the work after the dose averaging 98.1 per cent. of the normal, an absolute stimulation of 2 per cent., or a relative stimulation, as compared with the blank days, of 7.5 per cent. After syrup containing 1.2 gr. of caffein (also taken with carbonated water) the initial tendency to fatigue is again transformed into stimulation, this time resulting in a superiority of 9.5 per cent. over the blank days. After 3.6 gr. of caffein this relative stimulation rises to 10.6 per cent., and after 6 gr. there is an average superiority over the blank days of 8.0 per cent. The average of all the caffein doses shows clearly the same initial tendency to fatigue that is present on days with no doses, and also, in sharp contrast with these latter, the considerable stimulation following the dose (average absolute 4.0 per cent., average relative 9.4 per cent.).

The whole amount of this stimulation can not however be attributed to the caffein, since the plain syrup doses yielded an absolute stimulation of 2 per cent. The effect here is no doubt simply a case of sensory and psychic stimulation, produced partly by the excitement and interest of taking a drink with unknown contents, and partly by the sensory effects of the carbonated water, the act of swallowing, etc. To get the true caffein effect, then, this plain syrup effect must be allowed for, but the double control afforded by the blank days on the one hand and by the plain syrup days on the other, renders highly reliable the conclusion concerning the character of the caffein effect.

Since the net caffein stimulation is in this experiment present at

the 3:10 period the effect of the drug would seem to begin in about half an hour after taking the drink. Obviously this result is not comparable with that secured when the caffein is administered in capsules. Taken with the syrup the substance is already in solution and the reaction would be expected to follow more quickly than in the case of the capsule doses.

EXPERIMENT *B*

For further information as to the reaction time and for data concerning the duration and the after effect of the stimulation the results of the intensive experiment, in which the capsules were chiefly used, must be examined. Curves 14 to 17 give the average records for the various squads at each of the successive trials, on each of the three days, the solid line representing March 3, the broken line March 4 and the dotted line March 5.

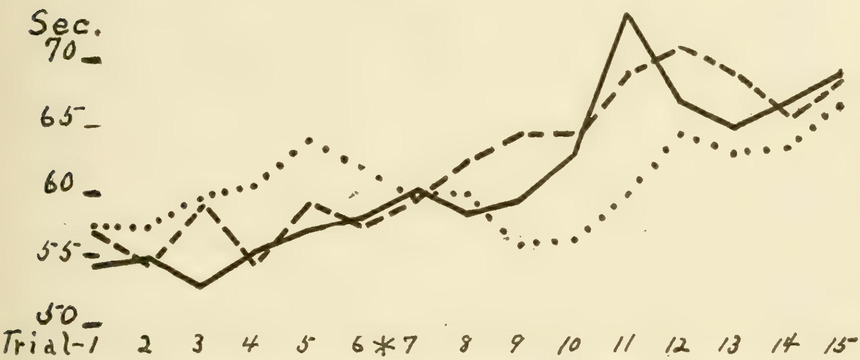


FIG. 14. COLOR-NAMING TEST. Experiment *B*. Squad I. (Control.)

Squad I. ran for the first two days on sugar doses only. The curves for these two days indicate in a very striking way the normal tendency to fatigue which this color-naming test shows. On the last day this squad received 3 gr. capsules of caffein after the 6th trial. At the 2d subsequent test (90 minutes later) the squad responds with a stimulation that is as pronounced as was the fatigue effect of the preceding days. After this strong stimulation the curve begins to rise again toward the normal fatigue level. But this level is never reached, for after the 12th trial a second capsule, containing 2 gr. of caffein was given, whereupon the curve drops again. A second stimulation thus follows upon the first, just as the curve is about to reach the fatigue level. Three hours after the first caffein influence begins to show itself there is still evidence of the primary stimulation produced by the 3 grain dose.

Squad II. took syrup with caffein (1.2 gr.) on March 3 and 5, and a sugar capsule on March 4. This last day shows the normal fatigue curve present also in Squad I. and in Experiment *C*. But 2–2½ hours after the caffein drink on March 3 the curve drops to a

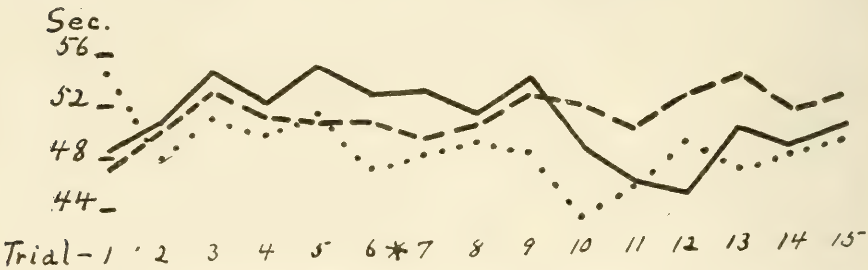


FIG. 15. COLOR-NAMING TEST. Experiment *B*. Squad II. (Solution.)

level lower than that of the performance before the dose. Evidence of this stimulation is still present at the close of the day, a persistence time of nearly 4 hours. On the following morning there is no evidence of relapse, the three curves starting at approximately the same level, with a slight superiority in favor of the last two days.[1]

FIG. 16. COLOR-NAMING TEST. Experiment *B*. Squad III. (3 gr.)

The second dose of caffein for this squad (March 5) does not show the same amount of stimulation that is present after the first dose. The general level of the curve drops but slightly, although the tendency is clearly to follow the level of the previous caffein day rather than that of the control day.

[1] This tendency of the curves to drop to lower general levels on the successive days is present with Squads III. and IV. as well. If it be recalled that on these intensive days the 100 colors were named in the same order at each of the 45 trials it will not be at all surprising that this slight appearance of general practise or familiarity is present.

With Squads III. and IV. (doses of 3 and 6 gr. respectively on March 3, and sugar doses on the remaining days) the results are not so apparent as those of Squads I. and II. The net result of the caffein on Squad III. seems to have been to relieve fatigue at the last tests of the day. This evidence is reenforced by the fact that the curve for the following day, although it begins on a lower level (probably as a result of the general practise pointed out above) finishes somewhat higher than that of the first day. Squad IV. behaves in much the same way as Squad III. The curve for the caffein day, beginning considerable higher up than that of the day after, finishes on about the same level. Fatigue on the third day is

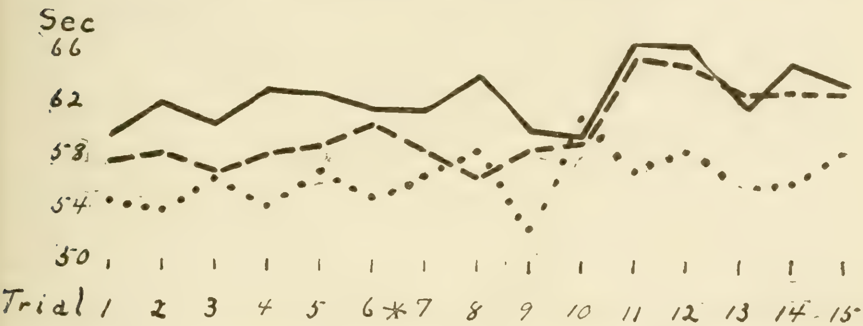


FIG 17. COLOR-NAMING TEST. Experiment *B*. Squad IV. (6 gr.)

slight. Perhaps the only safe inference to be drawn from the records of these two squads is the negative one, that caffein (in 3 and 6 gr. amounts) has at least no depressing effect on this test. The evidence from Squads I. and II. and from Experiment *C* is quite sufficient to demonstrate that when any effect is present it is stimulation.

SUMMARY

There is clear indication of stimulation in the color-naming test for the whole range of doses employed. But in all cases this stimulation is more apparent after the smaller doses than after the larger. The effect begins in about 2 to 2.5 hours after the capsule has been taken and is still present 3 to 4 hours later. There is no evidence of any after effect on the following day. Of incidental interest is the considerable retardation produced in this test by the evening meal. (See intensive curves.)

CHAPTER IX

The Influence of Caffein on the Opposites Test

In the opposites test, as in the color-naming test which precedes and in the calculation test which follows, there were no errors allowed, so that the only record to be made was the time of performance. In all cases the amount and quality of performance was constant. In the following curves of Subject 10 for Experiment *A*, a typical caffein effect is to be seen. The first two curves represent records made at the 12:00 period, before the dose was taken. At 5:30, four and a half hours after the dose, which was taken at the lunch hour, the second pair of curves results. The broken line represents days on which the dose was sugar, while the solid line represents the caffein days, the first two records being 1 gr., the next two 2 gr., and the following pairs 3, 4 and 6 grains. This subject is seen to be stimulated by the smaller doses of caffein, the solid line running lower at the 5:30 period than does the curve for control days. In the case of the larger doses there is no clear evidence of stimulation on the part of this subject. These individual curves are given only as suggestive of the character of the caffein influence for a more complete knowledge of which the results from the squad averages are given in the following.

Experiment *A*

Tables XXXVIII.–XLV. give the results of Experiment *A* for all four squads. The data here are treated in the same way as were those in the case of the tapping test, the actual records on caffein days being compared with calculated records for the same days. These calculated records are secured by averaging the records of the preceding and the following control day at the corresponding trial. The records for the respective doses are then averaged, and the difference between actual and calculated records computed. Cases in which the control records were longer are marked +, and cases in which the control records are quicker are marked —. After the dose has been taken, on a given day, the + differences will indicate stimulation, while the — differences will indicate retardation. But at the trials before the dose, the reverse will be the case, since the morning records of control days follow upon the caffein doses of the preceding days.

TABLE XXXVIII

OPPOSITES. SQUAD I., EXPERIMENT *A*

	February																		
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
7:45	56.0	50.5	51.1	49.9	46.4	45.2	41.6	42.1	44.7	42.2	40.7	42.5	45.3	40.9	36.6	41.6	37.6	36.4	37.8
10:00	54.9	50.9	47.9	49.9	46.9	43.5	45.5	41.5	44.5	42.0	43.7	44.5	38.9	38.8	37.5	38.6	35.1	36.5	35.2
12:00	54.8	51.2	51.7	48.1	46.1	42.7	44.7	42.5	42.2	46.7	42.3	42.6	39.1	38.5	41.6	37.7	37.7	36.9	36.8
1:00	Dose, sugar doses only, daily.																		
3:10	54.9	54.0	54.7	56.3	47.5	48.8	49.7	46.9	43.5	47.3	43.3	45.7	41.9	42.9	38.1	40.9	42.3	43.1	34.8
5:30	58.2	50.9	51.5	55.1	56.3	50.3	46.3	47.3	43.1	46.2	46.7	41.4	42.3	45.5	39.6	40.9	44.4	40.4	36.3

TABLE XXXIX

OPPOSITES. SQUAD II., EXPERIMENT *A*

	February																			March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	54.9	58.1	*53.0*	*48.2*	*49.1*	44.7	41.8	*42.1*	*42.7*	*43.2*	46.4	40.5	39.8	*35.7*	*38.4*	*35.8*	40.0	36.8	33.6	*37.3*	31.8
10:00	52.4	54.9	*51.1*	*45.2*	*44.9*	44.0	45.0	*41.1*	*39.6*	*39.7*	43.1	35.7	34.9	*36.7*	*39.2*	*36.1*	34.3	38.3	35.9	*35.0*	34.7
10:30	S	S	*1 gr.*	*1 gr.*	*1 gr.*	S	S	*2 gr.*	*2 gr.*	*2 gr.*	S	S	S	*4 gr.*	*4 gr.*	*4 gr.*	S	S	S	*6 gr.*	S
12:00	59.7	47.8	*52.7*	*45.7*	*48.7*	51.1	42.3	*40.2*	*47.1*	*43.1*	38.9	39.7	37.0	*36.7*	*36.3*	*43.5*	38.7	36.8	39.8	*34.8*	34.7
3:10	54.3	47.7	*49.6*	*50.5*	*47.0*	45.7	47.8	*40.5*	*41.8*	*44.0*	41.5	40.1	42.1	*38.7*	*34.0*	*36.9*	36.2	34.9	35.1	*33.7*	36.5
5:30	57.9	54.7	*50.8*	*43.5*	*53.4*	44.8	44.5	*43.7*	*40.7*	*43.9*	38.4	41.2	39.4	*35.4*	*35.8*	*38.1*	39.5	36.8	37.5	*36.7*	39.3

TABLE XL

Opposites. Squad III., Experiment *A*

	February																		March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	68.2	*60.5*	62.0	*53.7*	55.1	*50.7*	52.1	*43.7*	46.4	*44.2*	43.8	*41.8*	39.5	*41.7*	38.1	*35.4*	35.3	*37.4*	33.9	*37.3*	33.7
10:00	61.5	*59.8*	57.5	*56.1*	50.4	*46.9*	50.6	*46.1*	43.9	*43.7*	48.2	*45.2*	36.9	*37.7*	40.4	*36.5*	35.2	*36.9*	37.2	*37.9*	33.9
12:00	66.6	*58.1*	62.8	*53.1*	51.6	*51.7*	51.5	*46.5*	44.4	*47.3*	43.3	*41.6*	41.1	*40.3*	42.1	*39.0*	35.1	*36.6*	34.3	*36.9*	36.1
1:00	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	63.7	*56.7*	54.1	*58.1*	50.0	*50.3*	46.4	*44.7*	48.3	*45.7*	43.0	*40.4*	44.6	*45.0*	35.5	*33.5*	35.4	*37.7*	36.7	*32.1*	32.0
5:30	70.7	*62.4*	66.3	*54.5*	57.1	*50.9*	48.9	*47.3*	51.1	*45.5*	43.3	*41.4*	42.3	*41.4*	39.6	*38.7*	37.4	*39.5*	36.3	*35.4*	38.1

TABLE XLI

Opposites. Squad IV., Experiment *A*

	February																		March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	50.6	*48.4*	45.7	*44.8*	41.9	*40.6*	41.9	*42.0*	37.8	*39.1*	35.8	*36.4*	34.6	*35.9*	32.9	*35.9*	33.9	*33.8*	31.7	*34.1*	31.6
10:00	48.4	*46.1*	42.6	*47.2*	42.5	*41.4*	41.8	*39.2*	39.4	*40.6*	36.9	*36.5*	34.7	*35.1*	36.9	*33.3*	31.9	*34.1*	32.2	*31.7*	32.7
12:00	46.8	*48.7*	47.6	*46.8*	42.1	*42.4*	40.8	*38.6*	38.6	*40.0*	34.9	*38.0*	34.8	*39.9*	33.7	*34.4*	34.1	*38.2*	33.2	*32.2*	32.2
1:40	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	47.6	*44.9*	44.7	*43.5*	40.9	*41.9*	41.8	*38.9*	37.5	*36.6*	37.9	*39.4*	34.9	*35.6*	35.8	*34.1*	35.0	*32.9*	33.0	*34.1*	31.7
5:30	51.6	*46.8*	46.7	*43.5*	51.8	*43.4*	39.9	*38.6*	39.9	*39.2*	36.4	*35.2*	37.9	*37.6*	37.2	*34.4*	36.1	*33.0*	33.8	*32.8*	32.9

TABLE XLII

OPPOSITES. SQUAD I

	7:45			10:00			12:00		1:00		3:10			5:30	
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
51.2	50.2	+ 1.0	49.4	50.4	— 1.0	51.1	49.7	+ 1.4	S	53.0	55.2	— 2.2	54.4	53.0	+ 1.4
43.6	43.7	— 0.1	45.6	42.5	+ 3.1	44.5	42.6	+ 1.9	S	47.6	47.9	— 0.3	48.0	48.8	— 0.8
42.9	42.4	+ 0.5	42.7	43.3	— 0.6	41.5	44.7	— 3.2	S	43.0	46.5	— 3.5	44.7	43.8	+ 0.9
40.9	40.9	0.0	38.2	38.8	— 0.6	40.8	38.5	+ 2.3	S	40.0	42.9	— 2.9	40.9	45.5	— 4.6

TABLE XLIII

OPPOSITES. SQUAD II., EXPERIMENT *A*

Doses at 10:30 A.M. on successive trios of days

Time	7:45	10:00	12:00	3:10	5:30	No. of Days
Control days	45.3	43.1	44.3	43.6	44.7	11
1 gr. caffein	50.1	47.1	49.0	49.0	49.2	3
2 gr. caffein	42.7	40.1	43.5	42.1	42.8	3
4 gr. caffein	36.6	37.3	38.8	36.5	36.4	3
6 gr. caffein	34.6	34.9	34.8	35.1	38.0	1
Caffein Av.	41.0	39.8	41.5	40.7	41.6	10

TABLE XLIV

OPPOSITES. SQUAD III., EXPERIMENT *A*

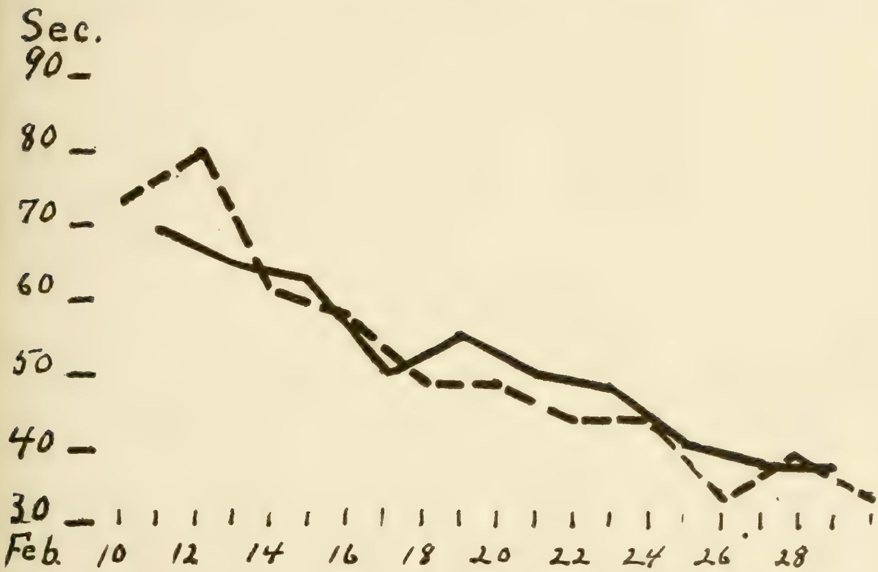
Sug. Av.	7:45 Caf. Av.	Dif.	Sug. Av.	10:00 Caf. Av.	Dif.	Sug. Av.	12:00 Caf. Av.	Dif.	1:00 Dose	Sug. Av.	3:10 Caf. Av.	Dif.	Sug. Av.	5:30 Caf. Av.	Dif.
61.9	57.1	+ 4.8	56.8	57.9	— 1.1	61.0	55.6	+ 5.4	1 gr.	55.5	57.4	— 1.9	65.1	58.5	+ 6.6
51.5	47.2	+ 4.3	48.9	46.5	+ 2.4	49.8	49.1	+ 0.7	2 gr.	47.8	47.5	+ 0.3	51.5	49.1	+ 2.4
43.4	43.0	+ 0.4	44.4	44.5	— 0.1	43.1	44.5	— 1.4	3 gr.	44.8	43.1	+ 1.7	45.0	43.5	+ 1.5
37.8	38.6	— 0.8	38.3	37.1	+ 1.2	40.1	39.7	+ 0.4	4 gr.	37.8	39.3	— 1.5	39.8	40.1	— 0.3
34.2	37.4	— 3.2	35.9	37.4	— 1.5	35.0	36.8	— 1.8	6 gr.	35.3	34.9	+ 0.4	37.1	37.5	— 0.4

TABLE XLV

OPPOSITES. SQUAD IV., EXPERIMENT *A*

Sug. Av.	7:45 Caf. Av.	Dif.	Sug. Av.	10:00 Caf. Av.	Dif.	Sug. Av.	12:00 Caf. Av.	Dif.	1:45 Dose	Sug. Av.	3:10 Caf. Av.	Dif.	Sug. Av.	5:30 Caf. Av.	Dif.
46.0	46.6	— 0.6	44.1	46.7	— 2.6	46.1	47.8	— 1.7	1 gr.	44.5	44.2	+ 0.3	49.3	45.2	+ 4.1
40.9	41.3	— 0.4	41.4	40.3	+ 1.1	40.6	40.5	+ 0.1	2 gr.	40.6	40.4	+ 0.2	42.9	41.0	+ 1.9
36.0	37.8	— 1.8	37.0	38.6	— 1.6	35.9	39.0	— 3.1	3 gr.	37.1	38.0	— 0.9	37.7	37.2	+ 0.5
33.6	35.9	— 2.3	35.1	34.2	+ 0.9	34.1	37.2	— 3.1	4 gr.	35.4	34.9	+ 0.5	37.2	36.0	+ 1.2
32.3	34.0	— 1.7	32.3	32.9	— 0.6	33.2	35.2	— 2.0	6 gr.	33.2	33.5	— 0.3	34.2	32.9	+ 1.3

Turning now to an examination of these tables, the records for Squad I. show purely chance distribution of differences at all but the 3:10 period, the magnitudes being small and the signs as often + as —. But at 3:10 there is a tendency for the pseudo-caffein days to be somewhat slow, the differences between actual and calculated records being all negative, and averaging about 2.2 seconds, the time of performance ranging between 42 and 55 seconds. Since this squad had only sugar doses throughout the experiment, this apparent retardation on pseudo-caffein days must either be considered simply a chance affair, or else to point to some more general



OPPOSITES TEST. Subject IX. Experiment *A*. Taking caffein at 1:00 P.M., with lunch. Broken line represents control days and solid line caffein days.

FIG. 18. Test at 12:00 M. No difference between caffein and control days.

factor that happened to affect performance in this test at the period and on the days in question. In the latter case, the fact should be borne in mind in interpreting the results secured from the other squads.

Squad II. yielded the records found in Table XLIII. The calculation of actual and expected records is not carried out here, since the doses were alike on successive trios of days, as explained in Chapter II. Instead the averages for all periods on each type of day (control, 1 gr. caffein, 2, 4 and 6 gr. of caffein) are given. The dose was taken at 10:30 A.M. The records for control days show

slight inferiority at the morning trial (7:45) and the best record of the days at 10:00. After this time the record lies always between these two extremes, with no considerable variation. Comparing this with the caffein days, the drug seems in no way to have influenced the speed of performance. The caffein averages show the same relation to each other for all doses, with no clear indication of any difference in performance after the dose.

In the case of Squad III., the 3:10 records show no consistent differences between caffein and control days, but the superiority of control days, found in Squad I., is not present here. By the time of

FIG. 19. Test at 5:30 P.M. Stimulation from small doses. No apparent effect from larger amounts.

the 5:30 test, caffein doses of 1–3 gr. yield considerable stimulation (6.6, 2.4 and 1.5 sec.). For larger doses the differences balance. The effect seems to decrease throughout with the magnitude of the dose. On the post-caffein mornings there appears to be retardation for the small doses of the day before, while the larger doses seem to result in stimulation at this late period. At 10:00 and 12:00 the differences are quite balanced. This squad took doses at the lunch hour.

With Squad IV., taking doses in mid-afternoon, the differences at the 3:10 period average less than .5 sec., and the signs balance, showing no effect unless the absence of the retardation found in the

case of the control squad be taken to be significant. At 5:30 there is stimulation for all doses, averaging 1.8 sec. or 4.5 per cent. The amount, as in the case of Squad III., is less for the large doses than for the smaller. On the following morning there is continued tendency to stimulation, especially marked at the 7:45 and 12:00 periods.

The effects of caffein on the opposites test in Experiment *A* are much more clearly shown if the standard of performance be taken

TABLE XLVI

OPPOSITES. SQUAD I., EXPERIMENT *A*

Showing the ratio between the records after the dose to the records before the dose. Comparing control days with pseudo-caffein days, for purposes of controlling data from the other squads

Subject	Control Av.	M.V.	Pseud. Caf. Av.	M.V.
1	1.041	.052	1.087	.048
4	1.007	.055	1.065	.038
7	.961	.044	1.066	.095
15	1.158	.090	1.133	.074
Average	1.041	.060	1.087	.063

TABLE XLVII

OPPOSITES. SQUAD III., EXPERIMENT *A*

Comparing the average record after the dose with the average record before the dose, on both control and caffein days

Subj.	Control Average	M.V.	Caffein 1 Gr.	2 Gr.	3 Gr.	4 Gr.	6 Gr.	Caffein Average	M.V.	Cont. Av.—Caf. Av.
3	.986	.050	1.017	.991	.903	1.063	.994			
			.888	.986	.952	1.076	.929			
			.952	.988	.927	1.069	.961	.979	.050	—.007
9	1.019	.083	1.150	1.050	.943	.853	1.040			
			.785	1.020	1.015	.972	.865			
			.967	1.035	.979	.912	.952	.969	.087	—.050
14	1.018	.048	.968	1.046	1.003	1.111	1.106			
			1.021	.990	.972	.997	.923			
			.994	1.018	1.037	1.054	1.014	1.023	.054	+.005
Sqd. Av.	1.008	.060	.971	1.013	.981	1.011	.975	.990	.063	—.018

to be the average performance before the dose, instead of the corresponding record on control days. In the latter case the true influence is perhaps partially concealed by the large practise effect on this test. Using the performance before the dose as the standard, and computing the ratio of the performance after the dose to this standard, for all types of day, yields the figures given in Tables XLVI.–L. In the case of Squad II., where there were three trials

for each sized dose, the average and M.V.'s of these three trials are used, and the two earlier and the last trial of the afternoon are kept separate. In the case of Squads III. and IV., who had only two days for each dose, both records are given, as well as the averages. In the tables the difference between the average ratios on control days and the ratios on caffein days are also given. The control squad (Table XLVI.) yields the same appearance of retardation on pseudo-caffein days that was pointed out in the preceding discussion of that squad. If this effect be taken to indicate some common out-

TABLE XLVIII

Opposites. Squad II., Experiment *A*

Showing the ratio of the 12:00 and 3:10 trials (Av.) to the average of the 8:00 and 10:00 trials. Dose at 10:30

Subj.	Control Av. and M.V.		Av. and M.V. 3 1 Gr. Days		Av. and M.V. 3 2 Gr. Days		Av. and M.V. 3 4 Gr. Days		1 6 Gr. Day	Caf. Av.	M.V.	Cont. Av. —Caf. Av.
8	1.026	.070	1.066	.056	1.005	.058	1.065	.073	.941	1.019	.060	—.007
12	1.004	.088	.959	.050	1.001	.043	.968	.033	.889	.954	.045	—.054
13	.993	.090	.995	.096	1.053	.120	1.049	.103	1.020	1.029	.106	+.036
Av.	1.008	.083	1.006	.064	1.016	.072	1.027	.066	.950	.999	.064	—.009

TABLE XLIX

Opposites. Squad II., Experiment *A*

Showing the ratio of the 5:30 trial to the average of the 8:00 and 10:00 trials. 7 hrs. after the dose

Subj.	Control Av. and M.V.		Av. and M.V. 3 1 Gr. Days		Av. and M.V. 3 2 Gr. Days		Av. and M.V. 3 4 Gr. Days		1 6 Gr. Day	Caf. Av.	M.V.	Cont. Av Caf. Av.
8	1.050	.063	1.118	.043	.979	.026	.999	.110	1.023	1.029	.059	—.021
12	1.024	.110	1.049	.096	1.091	.130	.967	.073	1.019	1.019	.099	—.005
13	1.023	.070	.944	.060	1.149	.080	.995	.076	.997	1.021	.072	—.002
Av.	1.032	.081	1.037	.066	1.073	.078	.987	.086	1.013	1.023	.076	—.009

side factor influencing the records on these days, the amount of stimulation indicated in the tables for the caffein squads is much less than what was really present, by some 8 per cent.

Consistently with the results indicated by other tests, the squad taking the dose in the A.M. (Squad II., Tables XLVIII. and XLIX.) shows the least influence. The stimulation present with this squad comes from the larger doses. The average amount for the squad is .9 per cent. at both the earlier and later trials of the afternoon.

Squad III. (dose with lunch) yields an average stimulation of twice this amount (1.8 per cent.). Only the heaviest of the three subjects, No. 14 (193 lbs.) fails to be stimulated, and it is this individual whose records bring the squad average down so low as it is.

As would be expected from the results of other tests, Squad IV. shows the caffein influence most clearly and to the greatest degree. The individual averages show stimulation without exception, the amounts, on the basis of the average for all doses, ranging from 2 to 12.5 per cent. The squad average for all doses is a stimulation of almost 6 per cent. Four of the five subjects get the greatest stimulation from the small doses. This is perhaps to be at least partly

TABLE L

OPPOSITES. SQUAD IV., EXPERIMENT *A*

Showing ratio of trials after dose to trials before dose. Dose at 1:45 P.M. Average of 3:10 and 5:30 records compared with average of 8:00, 10:00 and 12:00 records

Subj.	Control Average	M.V.			Caffein			Caffein Average	M.V.	Cont. Av. —Caf. Av.
			1 Gr.	2 Gr.	3 Gr.	4 Gr.	6 Gr.			
5	1.079	.060	.990	1.023	.860	.914	.890			
			.963	.890	1.033	.972	1.007			
			.976	.956	.946	.943	.948	.954	.052	—.125
6	1.048	.041	.960	1.000	1.038	1.048	.948			
			.885	1.125	.973	.932	.983			
			.922	1.062	1.005	.990	.960	.987	.050	—.061
10	1.033	.041	.978	1.120	.935	1.067	1.004			
			.996	1.004	.917	1.075	1.048			
			.987	1.062	.926	1.071	1.026	1.014	.051	—.019
11	1.011	.047	.938	.993	1.023	.959	.893			
			.928	.850	1.038	.945	1.047			
			.933	.921	1.030	.951	.970	.961	.053	—.050
16	1.018	.043	.940	.933	.915	.953	.961			
			.992	1.053	1.151	.994	1.000			
			.961	.993	1.033	.973	.980	.988	.048	—.030
Sqd. Av.	1.038	.046	.955	.998	.988	.985	.976	.980	.050	—.058

explained by the fact that the larger doses were given at a later point on the practise curve, when the individual was approaching more nearly to his physiological limit. In this case a smaller measurable amount might well indicate a greater caffein influence. The three individuals showing the greatest stimulation are subjects 5, 11 and 6, whose weights are 105, 110 and 125 lbs. respectively. Subjects 10 and 16, who get less stimulation from the same doses weigh 157 and 174 lbs. respectively. The dependence of the magnitude of the caffein influence on body weight is thus once more demonstrated. Comparing the three caffein squads, the caffein influence

is seen to be greatest when the dose is taken in the P.M. without food substance; it is less when the dose is taken with the mid-day meal, and still less when taken in the middle of the forenoon.

Experiment *B*

The results of Experiment *B*, the three intensive days, are given in curves 20–23. Here, as in the case of the other tests, the solid line represents March 3, the broken line March 4 and the dotted line March 5. Squad I. (control), Curve 20, shows clear indication of caffein stimulation. The first two days (sugar doses only) yield fatigue curves, the performance decreasing in speed from 35 seconds in the first two trials to an average of 45 seconds at the last two trials, a fatigue of 28 per cent. of the original time of performance. But on March 5, after the 3 gr. capsule of caffein after the 6th trial, the curve sweeps downward instead of upward,

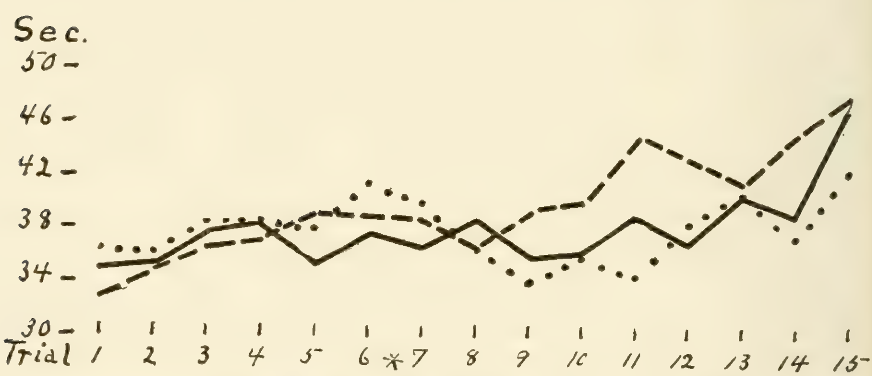


Fig. 20. Opposites Test. Experiment *B*. Squad I. (Control.)

being, at the 9th trial, 2.5 hours after the dose, several seconds lower than the original performance at the morning trials. This effect remains during three trials (2.5 hours). After this point the curve approaches the normal fatigue level, but is brought down again by the additional 2 gr. dose late in the evening. Before the dose the curves pursue quite the same level, but after the dose the trials for the rest of the day average 39.4 seconds on the first control day and 42.0 seconds on the second, as compared with 35.8 seconds on the caffein day.

In the curves for Squad II., all three days start out on about the same general level. On March 3 the curve then starts upward, as though the normal fatigue already seen in Squad I. were taking

place. But about 1 hour after the glass of syrup, carbonated water and 1.2 gr. caffein, taken after the 6th trial, the curve sweeps downward and remains low for the rest of the day, the second and fourth records from the end being the best of the whole day. On the following day (sugar dose after 6th trial) the fatigue effect is clearly present, the gradual rise being from a level of 30 seconds to one of

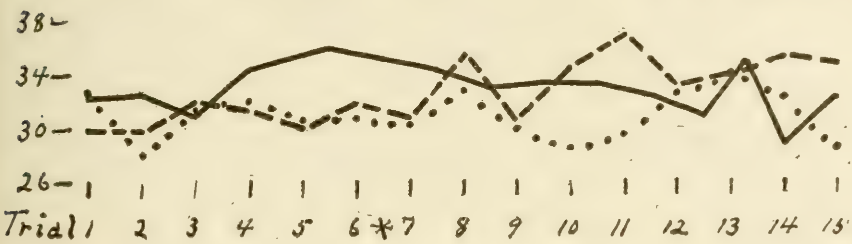


FIG. 21. OPPOSITES TEST. Experiment *B*. Squad II. (Solution.)

over 35 seconds. On the third day (caffein in syrup again after 6th trial) the tendency to fatigue is once more counterbalanced by an absolute stimulation beginning about 2 hours after the drink was taken.

The March 3d curve for Squad III. runs on a uniform level of 32 seconds until just before the dose, when the level drops to 30 seconds. After the 3 gr. caffein capsule between the 6th and 7th trials,

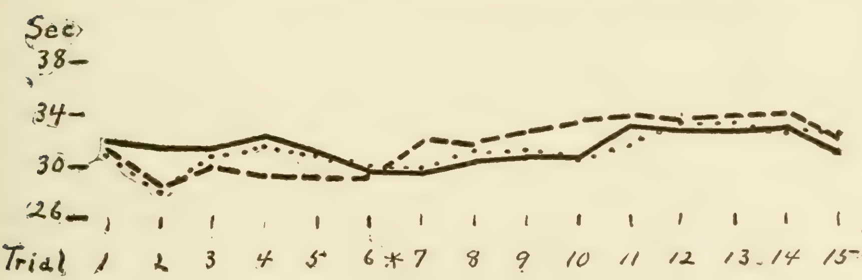


FIG. 22. OPPOSITES TEST. Experiment *B*. Squad III. (3 gr.)

the curve rises slowly until it again reaches the 32 second level, where it remains for the rest of the day. No evidence of fatigue is present. On the next day (sugar capsule after 6th trial) the curve runs on a level of about 29 seconds, some 3 seconds below that of the first morning, beginning to rise at about the time of the sugar dose, and continuing by a gradual ascent until a constant level of 33 seconds is reached—a fatigue of 14 per cent. On the second control day the general tendency is again fatigue, amounting to about 3

seconds (10 per cent.) at the final four trials. Whereas the performance before the caffein dose is slower than that on either of the control mornings, the records after this dose are uniformly better than those of the corresponding periods on control days. The morning trials after the caffein day give the quickest records made during the three days' work. That is to say, the suggestion of Experiment *A* that stimulation is present on the morning after the dose, is further confirmed by the behavior of Squad III. in this intensive experiment.

Squad IV. does not offer such a clear case of stimulation, and this is quite in accord with the results of Experiment *A*, where it was found that the greatest stimulation came from small doses, and where, in fact, the 6 gr. dose did not produce stimulation until the

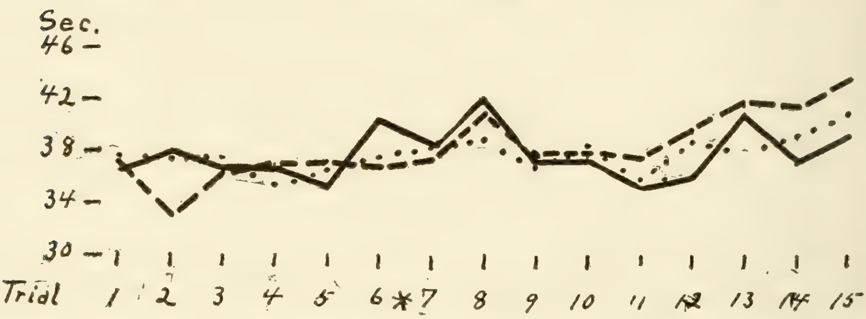


Fig. 23. Opposites Test. Experiment *B*. Squad IV. (6 gr.)

following morning except with one squad. Generally speaking, it is true of Squad IV. in this Experiment *B*, that whereas the records before the dose and up to three hours or so after the dose follow quite the same level on all three days, after this point the records on the caffein day are 3 to 4 seconds lower than those of the first control day and somewhat lower than those of the second. By far the quickest record made during the three days by this squad occurred on the morning after the caffein dose. The two control days both show fatigue at the last four trials of the day, but on the caffein day none of these records are slower than those made earlier in the day. Thus, for the 6 gr. dose, the effect is the same in kind but less in amount, as compared with that found with Squads I., II., and III.

Experiment *C*

The results of Experiment *C*, given in Table LI., show no reliable difference between the various types of days. An interesting point in this connection is the absence of sensory stimulation in these mental

processes, as compared with the considerable stimulation found on the plain syrup days in the case of the motor tests. In the case of the opposites test the syrup days do not differ appreciably from the

TABLE LI

OPPOSITES. EXPERIMENT *C*

Giving the average at each hour of the day on the various types of day, the average of the first three trials (before the dose), the average of the last two trials (after the dose) and the ratio of the latter to the former. Dose taken 15 minutes before each of the afternoon trials. Records are averages of 12 subjects.

Dose	Date March	8:00 Sec.	10:00 Sec.	12:00 Sec.	3:10 Sec.	5:30 Sec.	Av. of First 3 Sec.	Av. of Last 2 Sec.	Ratio of Last 2 to First 3 Per Cent.
Blank	6	34.0	32.5	32.2	33.5	34.7	32.9	34.1	103.0
days	11	34.1	33.5	34.3	34.6	34.5	34.0	34.6	101.8
						Av.	33.5	34.4	102.7
Syrup	7	35.7	33.4	35.7	34.4	34.6	34.9	34.5	98.8
days	9	33.1	33.0	33.1	35.1	33.4	33.1	34.3	103.6
						Av.	34.0	34.4	101.1
1.2 gr. caf.	8	33.5	34.9	34.3	34.0	34.2	34.2	34.1	99.7
3.6 gr. caf.	10	32.8	34.0	33.6	33.7	31.6	33.5	32.7	97.6
6 gr. caf.	12	33.9	33.3	35.9	36.5	35.6	34.4	36.1	104.9
						Av.	34.0	34.3	100.8

blank days, two days yielding the same percentage of fatigue (3.6 per cent.) and the other two differing only slightly (blank 101.8 per cent. and syrup 98.8 per cent.). In the case of the 1.2 gr. and 3.6 gr. caffein days, there is again no very clear difference as compared with the blank days or with the syrup days, although the averages turn out slightly quicker. The 6 gr. dose yields the highest percentage of fatigue of any of the 7 days, although the per cent. (4.9) is not reliably different from the large amount (3.6 per cent.) found on both blank and syrup days. On the whole there can not be said to be any caffein influence present, and the effect of sensory stimulation, if there at all, is but slight. The reason for the absence of caffein influence is no doubt to be explained by the fact that, as brought out in the results of Experiments *A* and *B*, the caffein does not begin to affect the processes involved in the opposites test until about 2 to 2.5 hours after the dose when taken in capsule form and about 1 hour after when taken along with syrup as in Experiment *C*. And in this experiment, the drink was taken only 15 minutes before beginning the tests. In the case of the motor tests, where the caffein influence begins more quickly and where sensory stimula-

tion is more effective, the results of Experiment *C* are much clearer than in the present case.

Summary

The influence of caffein on such processes as those involved in the opposites test is stimulation, which begins 1 to 2 hours after the syrup and 2.5 or 3 hours after the capsule dose. The amount of this stimulation, at its maximum, varies from 15 per cent. absolute stimulation to mere counterbalance of a normal fatigue tendency of about the same amount. In general the greatest effect results from the smaller doses. This stimulation is clearly present at the close of day, as much as 6–7 hours after the dose. In both Experiments *A* and *B*, and with all squads, there is evidence that the caffein influence is still operative on the forenoon following, 24 hours or more after the administration of the dose. The magnitude of the caffein influence varies inversely with body weight, and is relatively slight when the dose is taken at 10:30 A.M., somewhat greater when the caffein is taken along with the mid-day lunch, and still greater when the dose is taken in the middle of the afternoon, unaccompanied by food substance.

CHAPTER X

Influence of Caffein on the Calculation Test

The calculation test is more susceptible to the effects of practise than any other of the tests used. Even after the 35 preliminary trials of the first week of the experiment the record for Squad I. falls from as high as 102.7 seconds to as low as 61.2 seconds at the close of Experiment *A*,—a practise drop of 40 per cent. The other squads improve in a similar way, the drops ranging from the 60 per cent. of Squad I. to 32 per cent. for Squad II. This constant improvement through practise is so great as to obscure the caffein effect which the other tests yield in Experiment *A*. Thus the performance after a caffein dose might be speedier than the preceding day's record, but the same effect would be present solely as a result of practise, and there is no way of assigning proportionate results to the two factors. Moreover the records of the following sugar day would surpass those of the caffein day, simply as a result of further practise, and the general effect, when the practise effect is so considerable would not be distinguishable from the irregularities of a normal practise curve. Comparison of calculated with actual records will be consequently less significant than in the other tests, where the practise effect, after the preliminary week, is but slight. The three intensive days, however, occurred at a level of practise sufficiently uniform to yield most unmistakable and consistent results. For the sake of completeness and comparison the results of Experiment *A* are given, in spite of their relative ambiguity.

Experiment *A*

Tables LVI.–LIX. show, for Squads I., III., and IV., the comparison of calculated with actual records, computed as in the case of the Tapping Test, Chapter IV. Table LVI., for the control squad, shows balanced differences at 10:00, 12:00 and 3:10. At the 5:30 period, after sugar doses, the pseudo-caffein days show slightly lower records. The morning trials on pseudo-caffein days are longer, so that if this squad had really had caffein (instead of sugar doses daily) one might be tempted to infer stimulation after the caffein, followed by retardation on the following morning. For Squad III., taking doses of caffein at lunch hour on alternate days, the caffein

TABLE LII

Calculation. Squad I., Experiment *A*

	February																
	10 11	12	13	14	15	16	17 18	19	20	21	22	23	24	25	26	27	28
7:45	106.6	94.6	90.2	85.0	84.8	81.3	75.2	71.5	74.9	73.8	66.3	61.9	62.1	60.4	57.4	58.0	58.5
10:00	99.7	94.6	90.5	98.0	84.6	79.1	79.6	75.2	71.1	71.4	75.0	63.4	66.4	65.0	67.2	63.6	62.5
12:00	106.2	95.0	92.8	89.1	88.6	79.2	81.4	77.4	78.4	77.5	71.2	71.7	64.0	62.3	64.5	66.7	67.0
1:00	Sugar doses only, daily.																
3:10	107.1	100.3	94.0	91.5	87.3	84.9	84.8	79.5	73.2	78.0	75.4	67.6	55.8	69.3	64.1	63.3	60.1
5:30	117.1	105.6	100.2	96.5	92.9	84.1	79.9	77.0	77.4	75.7	80.7	72.2	70.0	71.5	65.2	67.9	62.6

TABLE LIII

Calculation. Squad II., Experiment *A*

	February																		March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	97.5	83.4	*83.0*	*76.3*	*80.5*	71.1	68.9	*66.9*	*63.5*	*59.9*	63.8	52.7	55.5	*52.2*	*49.3*	*48.3*	46.7	46.4	47.1	*48.3*	44.0
10:00	84.3	82.5	*81.4*	*75.6*	*68.1*	69.0	67.4	*65.5*	*67.1*	*54.6*	54.1	51.7	52.3	*51.4*	*49.9*	*50.1*	47.9	48.5	48.3	*46.6*	45.8
10:30	S	S	*1 gr.*	*1 gr.*	*1 gr.*	S	S	*2 gr.*	*2 gr.*	*2 gr.*	S	S	S	*4 gr.*	*4 gr.*	*4 gr.*	S	S	S	*6 gr.*	S
12:00	85.1	77.8	*76.7*	*82.2*	*71.7*	69.5	65.9	*62.9*	*60.9*	*62.0*	55.2	53.7	50.9	*54.6*	*50.7*	*48.0*	46.1	50.7	46.1	*46.4*	48.0
3:10	87.2	91.1	*81.3*	*75.2*	*75.8*	67.1	61.7	*67.8*	*60.3*	*57.3*	65.8	60.5	53.0	*54.3*	*48.1*	*51.3*	50.3	51.6	51.6	*42.5*	44.7
5:30	91.7	88.7	*96.5*	*75.4*	*77.4*	69.7	71.8	*71.1*	*61.5*	*59.8*	54.2	59.3	54.7	*54.3*	*50.9*	*57.0*	58.7	49.8	50.4	*48.6*	46.7

TABLE LIV

CALCULATION. SQUAD III., EXPERIMENT *A*

	February																			March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	88.3	*77.3*	76.2	*71.5*	74.1	*74.7*	65.6	*64.9*	66.6	*65.9*	58.5	*60.1*	57.9	*60.7*	55.6	*56.5*	55.8	*57.7*	54.6	*51.8*	58.4
10:00	83.6	*78.3*	75.5	*75.9*	77.7	*66.9*	66.8	*64.7*	69.1	*63.5*	63.1	*66.6*	58.5	*59.7*	56.1	*58.7*	54.6	*55.3*	56.0	*54.6*	56.7
12:00	83.5	*78.0*	79.9	*72.9*	68.9	*65.0*	68.3	*66.5*	66.3	*68.9*	63.0	*63.7*	59.4	*60.9*	55.7	*59.7*	63.3	*60.2*	59.2	*55.7*	56.1
1:00	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	85.0	*80.0*	74.1	*80.2*	71.9	*68.3*	71.7	*66.0*	67.5	*66.3*	63.0	*60.5*	61.9	*63.6*	64.9	*58.8*	57.5	*54.9*	58.1	*55.6*	55.7
5:30	87.9	*84.7*	75.5	*76.2*	72.1	*67.3*	73.9	*67.8*	67.7	*60.4*	64.7	*65.0*	66.5	*63.5*	62.1	*58.5*	63.3	*56.5*	56.8	*56.9*	54.2

TABLE LV

CALCULATION. SQUAD IV., EXPERIMENT *A*

	February																			March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	77.6	*73.0*	66.9	*67.4*	64.3	*62.1*	56.9	*61.9*	57.2	*55.4*	55.0	*51.9*	54.4	*49.2*	50.8	*49.0*	51.6	*46.3*	47.6	*46.5*	46.6
10:00	78.7	*73.2*	68.6	*69.0*	62.3	*61.4*	59.4	*61.7*	58.4	*58.8*	57.9	*57.0*	55.8	*54.6*	54.8	*52.1*	50.3	*51.3*	48.5	*50.9*	46.6
12:00	78.5	*71.3*	68.5	*68.2*	63.1	*65.8*	60.9	*62.6*	59.2	*59.9*	55.8	*54.1*	52.7	*52.0*	50.2	*54.6*	50.1	*49.1*	49.0	*50.3*	47.7
1:45	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	78.1	*74.4*	70.2	*69.5*	64.2	*63.3*	60.9	*57.8*	61.4	*57.9*	56.0	*56.8*	52.4	*52.1*	51.8	*49.9*	49.9	*47.9*	49.1	*48.2*	47.7
5:30	77.6	*73.4*	69.2	*69.6*	67.2	*67.9*	63.5	*59.7*	60.8	*57.6*	57.2	*53.3*	56.3	*53.3*	53.3	*50.6*	49.6	*49.2*	49.4	*49.9*	48.4

TABLE LVI

CALCULATION. SQUAD I

7:45 Sug. Av.	7:45 Caf. Av.	Dif.	10:00 Sug. Av.	10:00 Caf. Av.	Dif.	12:00 Sug. Av.	12:00 Caf. Av.	Dif.	1:00 Dose	3:10 Sug. Av.	3:10 Caf. Av.	Dif.	5:30 Sug. Av.	5:30 Caf. Av.	Dif.
93.0	89.8	+ 0.2	91.4	96.3	— 4.9	95.1	92.1	+ 3.0	S	95.7	95.9	— 0.2	102.7	101.1	+ 1.6
77.6	76.4	+ 1.2	78.8	77.2	+ 1.6	82.5	78.3	+ 4.2	S	82.6	82.2	+ 0.4	82.6	80.6	+ 2.0
69.1	68.2	+ 0.9	71.0	71.2	— 0.2	74.6	76.7	— 2.1	S	74.9	76.9	— 2.0	78.4	76.0	+ 2.4
62.6	61.2	+ 1.4	65.7	64.2	+ 1.5	67.5	67.0	+ 0.5	S	62.9	68.5	— 5.6	70.0	71.9	— 1.9

TABLE LVII

CALCULATION. SQUAD II

Trial	7:45	10:00	10:30 Dose	12:00	3:10	5:30	Trials
Averages	66.1	63.1	Sugar	62.5	65.6	67.5	11
at the	79.9	75.0	1 gr. Caf.	76.9	77.4	83.1	3
successive	63.4	62.4	2 gr. Caf.	61.9	61.8	64.1	3
trials	49.9	50.5	4 gr. Caf.	51.1	51.2	54.1	3
	46.2	46.2	6 gr. Caf.	47.2	43.6	47.7	1

TABLE LVIII

CALCULATION. SQUAD III

7:45			10:00			12:00			1:00	3:10			5:30		
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
78.8	74.4	+ 4.4	78.1	77.1	+ 1.0	78.1	75.5	+ 2.6	1 gr.	76.3	80.5	— 4.2	77.8	80.5	— 2.7
68.0	69.8	— 1.8	70.2	65.8	+ 4.4	68.0	65.8	+ 2.2	2 gr.	70.7	67.2	+ 3.5	71.9	67.6	+ 4.3
60.4	63.0	— 2.6	63.5	65.1	— 1.6	63.0	66.3	— 3.3	3 gr.	63.9	63.4	+ 0.5	65.9	62.7	+ 3.2
56.3	58.6	— 2.3	56.4	59.2	— 2.8	58.6	60.3	— 1.7	4 gr.	62.3	61.2	+ 1.1	63.5	61.0	+ 2.5
55.9	54.8	+ 1.1	55.9	55.0	+ 0.9	59.5	58.0	+ 1.5	6 gr.	57.4	55.3	+ 2.1	57.8	56.7	+ 1.1

TABLE LIX

CALCULATION. SQUAD IV

7:45			10:00			12:00			1:45	3:10			5:30		
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
69.0	70.2	— 1.2	69.6	71.1	— 1.5	69.7	69.8	— 0.1	1 gr.	70.7	72.0	— 1.3	70.9	71.5	— 0.6
58.9	62.0	— 3.1	59.9	61.6	— 1.7	61.1	64.2	— 3.1	2 gr.	61.9	60.6	+ 1.3	63.8	63.8	0.0
55.4	53.7	+ 1.7	57.6	57.9	— 0.3	55.9	57.0	— 1.1	3 gr.	56.5	57.4	— 0.9	57.9	55.5	+ 2.4
51.9	49.1	+ 2.8	53.8	53.3	+ 0.5	50.9	53.3	— 2.4	4 gr.	51.5	51.0	+ 0.5	53.2	52.0	+ 1.2
48.4	46.4	+ 2.0	48.5	51.1	— 2.6	49.0	49.7	— 0.7	6 gr.	48.6	48.2	+ 0.4	49.2	49.6	— 0.4

records are superior at both afternoon trials, except for 1 gr. doses. If this be taken to indicate stimulation there is no evidence of any sort of after effect on the following days. The morning trials do not differ from each other in any consistent way. The records for Squad IV. show no differences between caffein and control days for the trials after the doses. There is absolutely no evidence of stimulation so far as the figures are concerned. At 10:00 and 12:00 the signs of difference are all —, but the amounts are small. Perhaps the only conclusion to be safely drawn from the table is that there is no evidence of any deleterious caffein effect. Table LVII. gives for Squad II., for each of the five trials, the average performance on the various types of days. The general tendency (see Sugar days) is for the morning record to be slow, for the 10:00 and 12:00 records to be good, while the afternoon and evening trials return to the poorer level of the 7:45 test, or indeed are slightly inferior to that record. The records after 1 gr. of caffein show no departure from this tendency. But, after 2, 4 and 6 gr. doses at 10:30, there is a consistent tendency for the 3:10 record to improve rather than to fall. In fact after 2 and 6 gr. the 3:10 trial produces the best record of the day, and after 6 gr. the normal fatigue is clearly relieved, the usual falling off not coming until 5:30. All the evidence of this table, as of the preceding to a less degree, goes to suggest stimulation as the characteristic effect of caffein on the calculation test.

TABLE LX

Calculation. Squad I., Experiment *A*

Ratio between performance after dose to performance before dose

Subj.	Control Days	M.V.	Pseudo-Caffein Days	M.V.
1	1.044	.051	1.064	.038
4	1.019	.038	.973	.040
7	1.097	.035	1.075	.048
15	1.094	.064	1.117	.082
Average	1.051	.047	1.057	.052

The tendency of the caffein influence to be obscured by the practise effect, in this test, is much reduced when the standard of performance is taken to be the average record made before the dose, on any given day, rather than the corresponding records of other days. Tables LX.–LXIV. give the results for all squads, when this method of measurement is adopted. The ratio of the records after the dose to those before the dose, when these ratios for control days are compared with the ratios for caffein days, will afford a general view of the effect of the dose on the total afternoon's work. The only defect

of this method is that it gives no information as to the time relations of the drug influence, which information must come from the application of the preceding method and from the results of the intensive experiment. Table LX. gives the results of the control squad, computed by this ratio method. The two ratios (for control days and for pseudo-caffein days) are practically identical, such difference as is present (0.006) being in favor of the control days.

TABLE LXI

CALCULATION. SQUAD II., EXPERIMENT *A*

Average of 12:00 and 3:10 records compared with average of 8:00 and 10:00 records. Dose at 10:30

Subj.	Control Average	M.V.	1 Gr.	M V.	2 Gr.	M.V.	4 Gr.	M.V.	6 Gr.	Caf. Av.	Cont. Av. —Caf. Av.
8	1.031	.031	.981	.017	.962	.020	1.017	.043	.936	.974	—.057
12	.982	.031	.955	.120	.977	.073	1.011	.043	.986	.982	.000
13	1.010	.076	1.057	.056	1.025	.116	1.022	.050	.896	1.000	—.010
Av.	1.008	.046	.998	.074	.988	.069	1.016	.045	.939	.985	—.022

TABLE LXII

CALCULATION. SQUAD II., EXPERIMENT *A*

5:30 records compared with average record before dose

Subj.	Control Average	M.V.	1 Gr.	M.V.	2 Gr.	M.V.	4 Gr.	M.V	6 Gr.	Caf. Av.	Cont. Av. —Caf. Av.
8	1.134	.068	1.024	.040	.987	.043	1.050	.057	1.085	1.036	—.098
12	1.005	.080	.905	.060	1.063	.130	1.013	.026	1.015	.999	—.006
13	1.025	.101	1.265	.240	1.025	.086	1.158	.123	.976	1.106	+.081
Av.	1.055	.082	1.064	.113	1.026	.086	1.070	.068	1.025	1.046	—.009

Squad II. (Tables LXI., LXII.) yields, for the total range of doses, an average stimulation of 2.2 per cent. for the earlier tests. The slightest subject (No. 8, 144 lbs.) averages 5.7 per cent., while the other two (160 and 175 lbs.) show only a slight influence, No. 12, in fact showing no difference at all between the control and the caffein averages. By the time of the 5:30 record (Table LXII.) the balance of stimulation is only .9 per cent., subject 13 being poorer on caffein days. The greatest stimulation, with this squad, comes from the 6 gr. dose.

The three individuals of Squad III. all show stimulation. This effect is greatest for subject 9 (130 lbs.), less for subject 3 (159 lbs.) and still less for the heaviest subject, No. 14 (193 lbs.). The squad average is 2.1 per cent. stimulation for the total range of doses, and the stimulation does not appear until after the doses larger than 1 gr.

Squad IV. yields an average stimulation, for all doses, of 1.9 per

cent. The greatest stimulation is shown by subjects 5 and 6 (average weight 115 lbs.). Subjects 10 and 11 (average 133.5 lbs.) show somewhat less stimulation, while the heaviest individual, sub-

TABLE LXIII

CALCULATION. SQUAD III., EXPERIMENT *A*

Ratio between performance after dose to performance before dose

Subj.	Control Average	M.V.	1 Gr.	2 Gr.	3 Gr.	4 Gr.	6 Gr.	Caffein Average	Dif. Betw. Cont. and Caf.
3	1.063	.046	1.135	1.012	1.015	1.132	1.059		
			1.055	1.047	1.077	.995	1.058		
			1.095	1.029	1.046	1.063	1.058	1.058	—.005
9	1.024	.071	.997	.954	.861	1.024	.960		
			1.012	1.144	.866	.997	1.007		
			1.004	.999	.863	1.010	.983	.971	—.053
14	1.009	.050	1.037	.985	.999	1.000	.888		
			1.114	.989	.983	1.024	1.051		
			1.075	.987	.991	1.012	.969	1.007	—.002
Av.	1.032	.055	1.058	1.005	.966	1.028	1.003	1.012	—.021

TABLE LXIV

CALCULATION. SQUAD IV., EXPERIMENT *A*

Ratio between performance after dose to performance before dose

Subj.	Control Average	M.V.	1 Gr.	2 Gr.	3 Gr.	4 Gr.	6 Gr.	Caffein Average	Dif. Betw. Cont. and Caf.
5	1.012	.054	1.059	1.038	1.060	.973	.893		
			1.048	.969	.966	.929	Absent		
			1.053	1.003	1.013	.951	.893	.982	—.030
6	1.050	.040	1.020	1.089	1.049	1.071	.980		
			1.022	.945	1.000	.930	1.080		
			1.021	1.017	1.024	1.000	1.030	1.018	—.032
10	1.051	.045	1.008	1.054	1.035	.996	1.146		
			1.006	.980	.987	1.056	1.051		
			1.007	1.017	1.011	1.026	1.098	1.031	—.020
11	.978	.044	.942	.970	.943	.943	.967		
			.967	.933	1.099	.964	1.028		
			.954	.951	1.021	.953	.997	.955	—.023
16	1.004	.039	1.087	1.026	.900	1.121	.985		
			1.062	.905	1.042	.964	.998		
			1.074	.960	.971	1.042	.992	1.007	+.003
Av.	1.019	.044	1.021	.989	1.008	.994	.994	1.000	—.019

ject 16 (weight 174 lbs.), shows hardly any influence at all (.3 per cent. in favor of the control days). As in the case of Squad III. the greatest stimulation comes from the larger doses.

The mere suggestion of stimulation resulting from the previous method of treating the data, is thus not only most clearly confirmed by this second method, but the influence of body weight on the magnitude of the caffein influence is again made apparent. So far as this test is concerned there is no indication that the effect varies with the time of day nor with the conditions of administration of the dose.

Experiment *B*

The correctness of these conclusions is amply proven by the results of the three intensive days (Experiment *B*) given in the following curves. The first set of curves show the records for the control squad (I.) which ran for the first two days (March 3 and 4) on

FIG. 24. CALCULATION TEST. Experiment *B*. Squad I. (Control.)

sugar capsules. The curves for these two days are thoroughly representative of the normal tendency to fatigue in the calculation test. The amount of this fatigue rises to about 20 per cent. at the close of the control days. But on the third day (March 5) this squad received 3 gr. of caffein (capsule) after the 6th trial and 2 gr. more after the 12th trial. Up to the time of the first dose the normal fatigue tendency is clearly showing itself again. But at the 9th trial, 2.25 hours after the dose, this fatigue tendency is seen to be counterbalanced by a stimulation which carries the curve downward instead of upward, the subsequent records being considerably superior to those of the earlier trials. After the 2 gr. dose late in the evening, the record equals that of the best of the morning trials. Nothing can be said concerning the after effect of this stimulation

in the case of this squad, since this was the last day of the intensive experiment. That there is still a practise effect on the test as a whole is indicated by the tendency for the general level of the curves to drop lower and lower on successive days. This tendency is, however, by this time so slight that it is quite insufficient to obscure the caffein effect. Within a given day the normal tendency is fatigue rather than practise.

Squad II., on March 3, shows the characteristic fatigue curve beginning. After the 6th trial the dose was taken (1.2 gr. caffein with syrup and carbonated water). After the 7th trial (45 minutes later) the curve falls instead of continuing to rise, subsequent records being superior to any made before the dose and the final level being much lower than that of the morning. On the following day

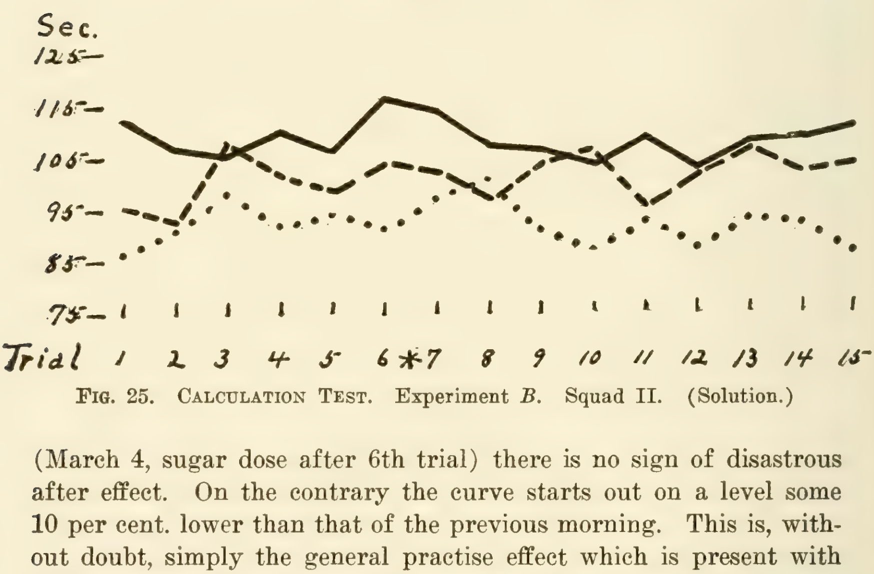


FIG. 25. CALCULATION TEST. Experiment *B*. Squad II. (Solution.)

(March 4, sugar dose after 6th trial) there is no sign of disastrous after effect. On the contrary the curve starts out on a level some 10 per cent. lower than that of the previous morning. This is, without doubt, simply the general practise effect which is present with all the squads. The curve for this second day, however, contrasts with that of March 3 by following the normal fatigue course so clearly demonstrated in the case of Squad I. In spite of the 10 per cent. superiority of the morning performance the afternoon and evening work is hardly better than that of the day before. On the 3d day (March 5) the normal fatigue curve begins again. This time the dose was once more a syrup with caffein. After the 8th trial (1.5 hr. after the dose) the curve sweeps downward at a most surprising rate, a stimulation of about 16 per cent. being present on the very last trials of the day, over 6 hours after the dose.

Squad III. (3 gr. caffein capsule March 3, sugar thereafter)

shows the same sort of influence. The control days afford fatigue curves of quite the normal shape, but the caffein curve fails to rise to the same degree as the day progresses, the best record of the day being made late in the afternoon, 3 hours after the dose. No after

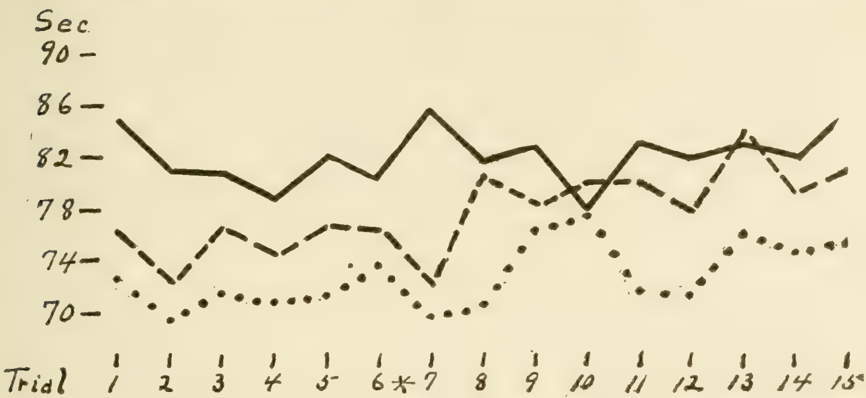


FIG. 26. CALCULATION TEST. Experiment *B*. Squad III. (3 gr.)

effect is observable. The general levels of the curves fall slightly from day to day, probably due again to simple practise effect.

If the effect of the 3 gr. caffein dose on Squad III. is obvious, the results of twice that amount on Squad IV. are doubly apparent.

FIG. 27. CALCULATION TEST. Experiment *B*. Squad IV. (6 gr.)

The 6 gr. dose was taken after the 6th trial on March 3. At the 9th trial, 2.25 hours after the dose, the record has fallen to 6 or 7 per cent. below the previous level and remains there for the rest of the day. On

the following morning, presumably as the result of general practise, the curve begins at about 10 per cent. lower than on the first day, gradually rising to show 8–10 per cent. fatigue by the time of the dose (sugar). But instead of falling off now, as did the caffein curve, the level continues to rise until the fatigue amounts to about 13 per cent. The afternoon and evening records of this second day are not superior to those of the caffein day preceding, in spite of the 10 per cent. advantage with which the day's work began. The second control curve (March 5) follows the familiar course of fatigue, the late records of the day being 15 or 16 per cent. inferior to those made earlier in the day.

Experiment *C*

The results of Experiment *C*, in which the doses were administered solely in liquid form, before each of the afternoon tests, are given in Table LXV. In order to show any possible individual differences the results are given separately for each subject. The average of the forenoon trials is taken as the norm for the day's work. The average of the two trials following the doses is compared with this norm, the table giving the afternoon average in terms of per cent. of the norm for the corresponding day.

TABLE LXV

Calculation. Experiment *C*

Giving performance after dose, in per cent. of work before dose

Subject		1	3	4	7	8	9	10	11	12	13	14	15	Average
2 blank		103	104	86	100	106	101	102	103	97	89	108	105	100.0
days		104	101	98	111	106	102	104	115	Abs.	124	103	112	107.2
	Av.	103.5	102.5	94	105.5	106	101.5	102	109	97	106.5	105.5	108.5	103.6
2 syrup		106	107	96	102	107	104	99	106	95	95	102	99	101.0
days		106	101	100	112	96	107	111	95	107	99	99	99	102.0
	Av.	106	104	98	107	101.5	105.5	105	100.5	101	97	100.5	99	101.5
1.2 gr. caf.		107	96	100	104	107	100	101	100	99	91	100	109	101.0
3.6 gr. caf.		104	98	89	104	99	105	99	94	100	106	92	99	99.5
6.0 gr. caf.		97	105	93	94	117	105	100	102	Abs.	122	108	99	103.1
Caffein Av.		102.7	99.7	94	100.7	107.7	103.3	100	98.7	99.5	106.3	100	102.3	101.2

On the blank days (no dose) the fatigue tendency amounts to an average of 3.6 per cent. for the two days, a much lower figure than was obtained in Experiment *B*, where there were 15 performances daily instead of 5 as in the present case. Only with two of the twelve subjects is there increased rapidity instead of fatigue. On plain syrup days the average performance shows slighter fatigue

(1.5 per cent.) than on blank days. In 10 of the 24 records there is absolute stimulation. Comparing, in the case of each individual, the syrup average with the average for the blank days, thus securing a measure of relative effect, a relative stimulation of 2 per cent. or over is found in 5 cases, but in the same number of cases the opposite effect is found. The plain syrup seems on the whole to quicken slightly the speed of performance.

The 1.2 gr. caffein dose yields an average record but .5 per cent. better than the plain syrup, or 2.6 per cent. better than the record for blank days. Such absolute fatigue (1 per cent.) as is present is due to the four individuals, 1, 7, 8 and 15, all but one of whom were retarded by the plain syrup doses, both absolutely and as compared with blank days.

The 3.6 gr. dose results in an absolute stimulation of .5 per cent., a record 4.1 per cent. better than that secured on blank days. The only exceptions to this stimulation are again subjects 1 and 7, along with 9 and 13, all but one of whom (13) showed retardation after plain syrup doses. After the 6 gr. dose the average amount of fatigue (3.1 per cent.) is only .5 per cent. less than the average for blank days, and is greater than that after plain syrup, 1.2 gr., and 3.6 gr. of caffein. Subjects 3, 8, 9, 13 and 14 show considerable absolute retardation and also relative retardation as compared with either the blank days or with the sugar days, although of these subjects 3, 8 and 14 had shown stimulation from smaller doses. Subjects 1, 4, 7, 10 and 15 show, on the other hand, considerable stimulation, both absolute and as compared with blank and syrup days. In fact, if the records of subjects 8 and 13 be omitted, the average performance for the squad after the dose is only 100.3 per cent. of the normal. The calculation test apparently affords instances of individual differences which may be taken up with profit in an appropriate place. It should be remembered that the 6 gr. of caffein, in this Experiment *C* was taken along with 5 ounces of the heavy syrup and two glasses of the carbonated water. This is much more syrup than was taken on plain syrup days. It would not be at all surprising if such a large amount of the syrup and water would fail to produce the sensory and psychic stimulation found when more agreeable amounts are taken.

Summary

The results of all four squads in Experiment *B* are thus found to confirm the suggestions of Experiment *A*. All squads reveal a most pronounced stimulation following caffein. This stimulation amounts

to a considerable per cent. of the initial performance, whereas the normal tendency, on control days, results in a corresponding degree of fatigue instead. The stimulation begins about 1 hour after the dose, when the caffein is taken in solution with syrup and carbonated water, and 2.25 to 2.5 hours after when the dose is in capsule form. The effect is still present at the close of the day's work, 6–7 hours after the dose. No evidence of any secondary depression is found. Instead the morning after the dose shows, without exception, a clear improvement over the work of the preceding morning. As pointed out above, it should perhaps be concluded that this superiority is due to general practise in the test. But it is interesting to find that this general practise effect, if such it be, is much greater after the caffein day than on the day following the sugar dose only. Another curious thing about this result is the further fact that the practise improvement, at least so far as the record shows it, seems to come during the night intermission, between the series of trials. The normal tendency at successive trials on the same day is fatigue rather than increase in speed. In fact there is considerable justification for supposing that the influence of the caffein dose, which is seen to be unmistakably present 6 or 7 hours after administration, is still operative on the following day. This operation might be in the form of a real persistence of stimulation or in the form of increased efficiency due to skill or disposition acquired during the stimulation of the preceding day. The former hypothesis would not be inconsistent with the data available concerning the length of time caffein may remain in the system after ingestion. If this be taken to be a real persistence, one is at once interested to know why the effect on motor processes seems to be so much more transient than that manifested in this more strictly mental performance. The results of Experiment *A*, when computed by the ratio method, afford further evidence of the fact that the magnitude of the caffein influence varies inversely with the body weight of the individual tested.

CHAPTER XI

The Influence of Caffein on Discrimination and Choice Reaction Times

In this test the average of 10 correct reactions to the blue disc was taken as constituting the record for a given period. Reactions to the red disc were recorded only as being larger or smaller than a stated magnitude. Since these red exposures served only as control stimuli and varied in number from test to test, they are of no particular interest or value. The reactions to the blue disc were measured in sigma, the mean variations of the 10 trials were computed, and the number of false reactions at each sitting noted.

Experiment *A*

In Experiment *A*, on February 13, at the 10 :00, 3 :10 and 5 :30 tests, an accident to the instrument rendered the reactions invalid. Hence for those three periods the records of the preceding day of the same type (1 gr. caffein) were substituted in computing the averages and in comparing actual with calculated records. Tables LXVI.–LXXIII. show for all squads of Experiment *A* the results from the point of view of discrimination time.

Squad I. (control) shows, as is to be expected, only chance differences between the control days and the pseudo-caffein days. For all hours of the day the differences between actual and computed records are divided in sign between + and —, in a random way, and the magnitudes of these differences are in most cases small.

Squad II. shows, on control days, at the trials after the sugar dose, a reaction time uniformly 92 per cent. of the daily normal (average of the two trials preceding the dose). For caffein doses of 1–4 gr. the reactions after the dose are considerably slower as compared with control days, ranging from 96 per cent. to 104 per cent. of the morning performance. In the case of the largest dose (6 gr.) this retardation is not present, the trials after the dose being 90 per cent. as quick as those before, an average which quite resembles that of the control days or is even a little better, suggesting stimulation instead. The general character of the caffein influence seems to be retardation except for the large dose. This squad took caffein on three successive days at 10 :30 A.M., in each case the caffein trio being followed by a corresponding number of control days.

TABLE LXVI

Discrimination Reaction. Squad I., Experiment *A*

	February																		
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
7:45	330.0	323.2	361.7	351.0	359.5	349.5	313.5	318.7	309.0	330.0	340.7	319.7	313.2	317.0	312.7	313.5	354.8	360.5	330.8
10:00	333.5	335.7	326.7	335.7	345.5	314.2	284.2	302.2	313.2	329.5	321.5	318.5	310.2	307.0	313.5	316.2	348.7	364.0	342.5
12:00	332.7	327.2	330.7	351.2	364.2	345.5	322.5	333.0	330.7	323.5	326.0	294.0	311.2	323.5	326.0	324.0	354.0	347.3	321.0
1:00	Sugar doses only, daily.																		
3:10	319.7	336.0	335.5	336.0	349.5	337.2	310.7	320.2	364.0	330.0	328.2	303.0	314.5	321.7	318.5	332.8	363.3	362.3	335.8
5:30	349.7	369.5	335.0	369.5	360.5	357.2	358.7	361.5	355.5	349.7	348.7	316.7	302.7	343.2	349.5	341.2	349.7	331.5	334.8

TABLE LXVII

Discrimination Reaction. Squad II., Experiment *A*

	February																			March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	303.3	302.0	*343.7*	*356.3*	*325.0*	304.3	304.0	*300.0*	*295.0*	*306.7*	317.0	285.3	280.7	*291.0*	*300.3*	*323.3*	327.0	356.0	325.7	*332.3*	278.3
10:00	319.3	283.7	*329.3*	*283.7*	*354.2*	294.3	288.7	*303.3*	*289.7*	*278.3*	281.3	290.3	266.7	*282.7*	*309.7*	*298.7*	309.3	374.3	303.0	*316.0*	320.0
10:30	S	S	*1 gr.*	*1 gr.*	*1 gr.*	S	S	*2 gr.*	*2 gr.*	*2 gr.*	S	S	S	*4 gr.*	*4 gr.*	*4 gr.*	S	S	S	*6 gr.*	S
12:00	308.0	227.7	*321.7*	*323.7*	*331.3*	292.0	306.7	*288.3*	*302.7*	*282.3*	281.3	283.7	272.0	*824.3*	*299.7*	*287.7*	317.7	329.3	327.7	*295.3*	298.0
3:10	322.3	306.3	*351.0*	*306.3*	*298.7*	297.0	285.7	*301.0*	*305.0*	*288.7*	279.0	280.3	277.7	*295.0*	*296.7*	*288.3*	327.0	345.0	315.7	*292.7*	299.7
5:30	306.0	335.7	*348.0*	*335.7*	*315.0*	309.3	316.7	*310.7*	*302.0*	*291.7*	299.0	283.7	275.3	*304.7*	*324.3*	*306.6*	315.0	301.3	307.0	*293.0*	307.0

TABLE LXVIII

DISCRIMINATION REACTION. SQUAD III., EXPERIMENT *A*

	February																		March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	309.0	*320.7*	361.0	*335.3*	335.3	*325.0*	320.3	*313.0*	310.0	*324.7*	335.0	*321.3*	290.0	*321.7*	342.3	*346.3*	340.3	*347.3*	344.0	*333.3*	327.3
10:00	325.3	*321.3*	357.0	*321.3*	342.7	*313.7*	338.0	*318.7*	317.0	*315.7*	328.0	*309.7*	340.3	*341.3*	332.0	*345.3*	354.0	*374.3*	337.7	*334.3*	330.7
12:00	322.7	*329.0*	306.7	*341.0*	344.7	*329.0*	332.7	*301.7*	320.3	*334.3*	321.3	*307.7*	327.3	*328.0*	331.7	*309.3*	350.0	*353.0*	322.0	*324.0*	326.0
1:00	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	329.3	*334.7*	334.0	*334.7*	335.3	*353.3*	338.0	*305.3*	306.0	*329.7*	303.3	*301.3*	309.3	*324.3*	319.7	*321.3*	345.0	*343.3*	346.3	*297.7*	344.7
5:30	311.7	*344.0*	340.3	*344.0*	346.0	*346.3*	343.3	*357.7*	324.7	*348.0*	324.7	*331.3*	322.3	*347.0*	348.7	*330.7*	342.0	*339.7*	326.0	*331.0*	341.0

TABLE LXIX

DISCRIMINATION REACTION. SQUAD IV., EXPERIMENT *A*

	February																		March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	321.2	*299.8*	344.8	*351.6*	331.6	*323.0*	313.8	*294.6*	312.8	*304.8*	302.2	*302.6*	295.6	*303.4*	295.6	*318.6*	337.6	*332.4*	347.6	*336.6*	323.0
10:00	295.4	*296.0*	347.0	*296.0*	323.8	*306.2*	314.6	*299.0*	321.4	*288.8*	306.0	*314.8*	316.2	*297.4*	317.0	*321.6*	336.2	*355.4*	352.4	*322.4*	326.2
12:00	294.4	*285.8*	339.8	*334.4*	300.6	*302.8*	297.6	*285.6*	342.4	*299.8*	290.2	*292.2*	288.4	*303.6*	306.2	*328.2*	339.2	*353.0*	322.6	*308.8*	317.0
1:45	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	293.2	*299.6*	320.6	*299.6*	324.0	*310.2*	312.4	*308.6*	320.4	*296.2*	308.8	*307.4*	297.0	*298.8*	325.8	*326.4*	336.4	*329.4*	349.8	*329.4*	344.0
5:30	308.0	*333.4*	323.6	*333.4*	315.2	*331.4*	308.4	*320.6*	314.0	*315.2*	308.4	*311.6*	305.6	*320.6*	332.0	*335.8*	342.8	*339.6*	342.2	*340.2*	322.4

TABLE LXX

DISCRIMINATION. SQUAD I., EXPERIMENT *A*

Sug. Av.	7:45 Caf. Av.	Dif.	Sug. Av.	10:00 Caf. Av.	Dif.	Sug. Av.	12:00 Caf. Av.	Dif.	1:00 Dose	Sug. Av.	3:10 Caf. Av.	Dif.	Sug. Av.	5:30 Caf. Av.	Dif.
353.3	337.1	+16.2	333.1	335.7	+2.6	339.6	339.2	+ 0.4	1 gr.	335.1	336.0	— 0.9	345.1	369.5	+24.6
323.9	334.1	—10.2	306.8	308.2	—1.4	335.5	339.3	— 3.8	2 gr.	333.8	328.7	+ 0.1	358.4	359.4	— 1.0
325.9	324.9	+ 1.0	316.7	324.0	—7.3	323.5	308.8	+14.7	3 gr.	333.8	316.5	+17.3	338.9	333.2	+ 5.7
323.4	315.3	+ 8.1	321.5	311.6	+9.9	329.3	323.8	+ 5.5	4 gr.	328.7	327.3	+ 1.4	337.9	342.2	— 4.3

TABLE LXXI

DISCRIMINATION. SQUAD II., EXPERIMENT *A*

Dose	Average of 7:45 and 10:00	12:00 Per Cent.	3:10 Per Cent.	5:30 Per Cent.	No. of Trials
Sugar	329.4	91	92	93	11
1 gr.	338.0	97	96	99	3
2 gr.	295.8	98	101	102	3
4 gr.	309.0	97	98	104	3
6 gr.	324.2	91	90	90	1

TABLE LXXII

DISCRIMINATION. SQUAD III., EXPERIMENT *A*

7:45			10:00			12:00			1:00	3:10			5:30		
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
341.6	328.0	+13.6	345.6	321.3	+24.3	320.2	335.0	—14.8	1 gr.	333.2	334.7	— 1.5	334.6	344.0	— 9.4
321.5	319.0	+ 2.5	334.0	316.2	+17.8	332.6	315.4	+17.2	2 gr.	329.4	329.3	+ 0.1	339.4	352.0	—12.6
317.5	323.0	— 5.5	328.4	312.7	+15.7	322.6	321.0	+ 1.6	3 gr.	305.5	315.5	—10.0	324.1	339.7	—15.6
328.8	334.0	— 5.2	339.6	343.3	— 3.7	335.2	318.7	+16.5	4 gr.	323.5	322.8	+ 0.7	340.5	338.9	+ 1.6
339.0	340.3	— 1.3	340.1	354.3	—14.2	330.0	338.5	— 8.5	6 gr.	345.6	320.5	+25.1	333.8	335.4	— 1.6

TABLE LXXIII

DISCRIMINATION. SQUAD IV., EXPERIMENT *A*

7:45			10:00			12:00			1:45	3:10			5:30		
Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.	Dose	Sug. Av.	Caf. Av.	Dif.	Sug. Av.	Caf. Av.	Dif.
335.6	325.7	+9.9	328.3	296.0	+32.3	318.7	310.1	+ 8.6	1 gr.	314.6	299.6	+15.0	317.6	333.4	—15.8
318.0	308.8	+9.2	318.6	302.6	+16.0	309.6	294.2	+15.4	2 gr.	317.3	309.4	+ 7.9	311.5	326.0	—14.5
303.2	303.7	—0.5	312.4	301.8	+10.6	302.8	296.0	+ 6.8	3 gr.	308.8	301.8	+ 7.0	309.1	313.4	— 4.3
306.1	311.0	—4.9	321.6	309.5	+12.1	310.0	315.9	— 5.9	4 gr.	321.3	312.6	+ 8.7	328.1	328.2	— 0.1
334.4	334.5	—4.4	339.0	338.9	+ 2.1	327.6	330.9	— 5.9	6 gr.	341.3	329.4	+15.6	336.3	339.9	— 2.7

In the case of Squad III., taking dose with lunch on alternate days, the 3:10 test, 2 hours after the dose shows no consistent differences between caffein and control days, except a strong suggestion of stimulation from the 6 gr. dose (difference + 25). This result is in line with the lack of retardation from 6 gr. in the case of Squad II. Indeed, in the case of that squad also, the record after 6 gr. was slightly better than on control days (90 per cent. of the morning average as compared with 92 per cent.). At the 5:30 period, in the case of this Squad III., all but one of the signs are —, denoting slower reactions on caffein days, the differences decreasing in magnitude with the larger doses, quite confirming, again, the results from Squad II. At 7:45 on the following morning the differences are (except for the smallest dose) very small (never over 5.5 sigma), and the signs balance. At 10:00 and 12:00 again there is no consistent difference between the two types of days. If there is any after effect at all here, it is a tendency to continued retardation from the small doses and possibly toward stimulation from the large doses which had on the previous day scarcely any effect. In general this squad suggests a retardation about 4.5 hours after the dose, with no clear evidence of after effect.

Squad IV., taking the dose on empty stomach in mid afternoon, shows at 3:10, clear evidence of stimulation for all doses. But by 5:30 this effect has given place to such retardation as that found in the case of Squad III. The amount of this retardation, as also in the case of Squad III., decreases in amount with the larger doses. On the following morning the small doses, which had produced retardation on the preceding evening, seem to show a persistence of this effect. The larger doses, which made but little difference at the 5:30 trial, show no clear effect on the following day. The retardation for small doses is still present at 10:00 and 12:00. The general effect of the caffein on this squad seems then to have been an initial stimulation, followed by retardation for small doses and by balance for large doses. This effect is still present during the following forenoon. The large dose seems to produce so much initial stimulation that the subsequent retardation which characterizes the smaller doses is absent. The same thing was found in the case of Squad III. In both these squads, when retardation is present, it decreases in amount for the larger dose, and when stimulation is present the reverse relation is the case. The only difference between Squad III. and Squad IV., in Experiment *A*, consists in the absence of the initial stimulation from small doses in the case of Squad III. Both these squads agree with Squad II. in showing retardation from small doses but not from large.

If now the standard of comparison be taken to be the performance before the dose, rather than the corresponding records on other days, and the comparisons be presented in the form of ratios, the results presented in Tables LXXIV.–LXXVIII. are obtained. In the case of the control squad (Table LXXIV.) two subjects show relative superiority for the control days and two for the pseudo-caffein days, the squad average being 1.7 per cent. in favor of the control days.

TABLE LXXIV

DISCRIMINATION. SQUAD I., EXPERIMENT *A*

Ratios of reaction times after dose to times before dose

Subj.	Control Av.	M.V.	Pseudo-Caf. Av.	M.V.
1	.999	.036	1.026	.033
4	1.035	.038	1.022	.037
7	1.059	.071	1.014	.065
15	1.020	.087	1.120	.052
Average	1.028	.058	1.045	.046

TABLE LXXV

DISCRIMINATION. SQUAD II., EXPERIMENT *A*

Ratios of times after dose to times before dose. 12:00 and 3:10

Subj.	Control Average	M.V.	1 Gr.	M.V.	2 Gr.	M.V.	4 Gr.	M.V.	6 Gr.	Caffein Av.	Dif
8	1.015	.047	.946	.070	.980	.023	.994	.023	.854	.943	—.072
12	.997	.060	.939	.040	1.063	.027	.973	.017	.928	.976	—.023
13	.955	.058	.925	.080	.937	.033	.947	.047	.945	.938	—.017
Av.	.989	.055	.933	.063	.996	.027	.971	.029	.909	.952	—.037

TABLE LXXVI

DISCRIMINATION. SQUAD II., EXPERIMENT *A*

Ratio of the 5:30 record to record before dose

Subj.	Control Average	M.V.	1 Gr.	M.V.	2 Gr.	M.V.	4 Gr.	M.V.	6 Gr.	Caffein Av.	Dif.
8	1.016	.072	.986	.047	.997	.027	1.047	.057	.766	.949	—.067
12	1.023	.048	.977	.077	1.076	.007	1.031	.053	.978	1.000	—.023
13	.971	.087	.883	.017	.993	.053	1.034	.100	.989	.975	+.004
Av.	1.003	.069	.948	.047	1.022	.029	1.037	.070	.911	.975	—.028

Squad II. shows, at the 12:00 and 3:10 periods (Table LXXV.), stimulation for all caffein doses, the average amount being 3.7 per cent., and the effect being greatest for the 1 and 6 gr. amounts. At 5:30 (Table LXXVI.) two of the subjects still show stimulation, while the third shows no stimulation except from the 1 gr. dose, the other doses being inferior to the performance on control days. Here again, in the squad averages, the stimulation comes from the 1 and

6 gr. amounts only, the 2 and 4 gr. producing retardation. In this case then, we have the only contradiction between the results of Experiment *A* as computed by the two methods. The first method gave, as the general effect of caffein on Squad II., retardation, except for the largest dose. The general suggestion of the present is stimulation. Since the two sets of results do not agree, we are unable to draw any conclusion whatever concerning the discrimination time for this squad. When the dose is taken in the morning, as was the case with this squad, there is thus no clear indication of any caffein influence on the discrimination reaction time. At first thought one might expect the squad averages of Tables LXXI. and LXXV. to be identical, but it must be remembered that one is the per cent. of averages, the other the average of per cents.

When the dose is taken along with lunch, as in the case of Squad III. (Table LXXVII.) the individuals do not agree. Subject 3 shows retardation from small doses and stimulation from large. The two other subjects show hardly any effect whatever from small doses, but considerable stimulation (2.5 per cent. and 4.6 per cent.)

TABLE LXXVII

DISCRIMINATION. SQUAD III., EXPERIMENT *A*

Ratio of records after dose to records before dose

Subj.	Control Average	M.V.	1 Gr.	2 Gr.	3 Gr.	4 Gr.	6 Gr.	Av. of 1 and 2 Gr. Also Difference betw. Control Av. and These	Av. of 4 and 6 Gr.
3	1.016	.050	1.063	1.264	1.117	1.025	.975	1.174	.979
			1.150	1.220	1.044	.974	.945	+.158	—.037
			1.106	1.242	1.080	.999	.960		
9	1.018	.060	1.072	1.039	1.034	.981	1.009	1.016	.983
			.972	.979	.999	.994	.951	—.002	—.025
			1.022	1.009	1.016	.987	.980		
14	1.011	.028	1.013	.977	.984	1.036	.911	1.005	.965
			1.021	1.010	.986	.963	.956	—.006	—.046
			1.017	.993	.985	.999	.932		
Av.	1.015	.046	1.048	1.081	1.027	.995	.957	1.064	.976
								+.049	—.039

from larger amounts. Although the squad average shows retardation from small doses and stimulation from large, the behavior of the individuals making up the squad suggests very strongly that when the substance is taken at meal-time the retardation which is otherwise likely to be present tends to disappear.

The results for Squad IV. are thoroughly consistent with the conclusions previously drawn. The squad average shows a retardation of 3 per cent. from 1 to 4 gr. doses, and a stimulation of 4.1 per cent. from 6 gr. The individuals all conform to this rule, with the exception of the heaviest subject (No. 16, 174 lbs.) who shows stimulation from all but the 3 and 4 gr. amounts. Two of the individuals, subjects 6 and 10 (weights 125 and 157 lbs.) get stimula-

TABLE LXXVIII

DISCRIMINATION. SQUAD IV., EXPERIMENT *A*

Ratio of records after dose to records before dose

Subj.	Control Average	M.V.	1 Gr.	2 Gr.	3 Gr.	4 Gr.	6 Gr.	Av. of 1–4 Gr. (Also Diff. betw. These and Control Average)	6 Gr. (Also Diff. betw. These and Control Average)
5	.993	.053	1.142	1.034	1.000	1.036	.959	1.053	.959
			1.007	1.063	.979	1.084	?	+.060	—.034
			1.074	1.048	.989	1.060	.959		
6	1.006	.043	1.100	1.087	1.110	1.024	.935	1.080	.976
			1.033	1.116	1.008	.917	1.018	+.074	—.030
			1.066	1.101	1.059	.970	.976		
10	1.014	.047	1.080	1.105	.953	1.012	.965	1.037	.982
			.993	1.099	1.073	1.000	.999	+.023	—.032
			1.037	1.102	1.013	1.006	.982		
11	1.006	.087	1.062	1.046	1.054	.947	.926	1.027	.934
			1.139	1.106	1.037	1.081	.943	+.021	—.072
			1.100	1.076	1.045	1.014	.934		
16	1.020	.044	.980	.907	1.020	1.080	1.019	.997	.984
			.991	.991	1.019	1.029	.950	—.023	.036
			.985	.949	1.020	1.054	.984		
Av.	1.008	.055	1.052	1.055	1.025	1.020	.967	1.038	.967
								+.030	—.041

tion from the 4 gr. dose. In the squad averages retardation is present up to the point of the 6 gr. dose, which yields stimulation. The amount of retardation, further, decreases with increase in amount of the dose, from 5.2 per cent. for 1 gr. and 5.5 per cent. for 2 gr. to 2.5 per cent. for 3 gr. and 2.0 per cent. for 4 gr., being transformed, at that point to stimulation for the 6 gr. The magnitude of the effect is again dependent on the body weight of the individuals, and is greater for subjects 5, 6 and 11 (105, 125 and 110 lbs.) than for subjects 10 and 16 (157 and 174 lbs.).

Experiment *B*

The results of Experiment *B* are given in the following curves 28–31. In this experiment special care was taken to keep the illumination of the room constant (a factor which had been somewhat variable in Experiment *A*.) All the trials were made in a room artificially lighted by using gas mantles which gave a fairly pure white light.

Squad I. (control, sugar first two days, caffein third day, dose after 6th trial) shows on control days, only a slight inclination toward inferior reactions in the latter part of the day. After the caffein dose (3 gr.) on the third day, there is possibly a slight retardation late in the evening, but this retardation, if present, is not constant, for it shows at only two or three of the trials. The final records of the day and the majority of the trials after caffein are as

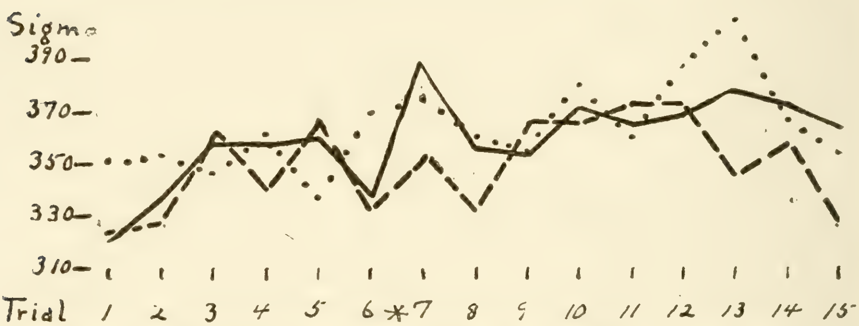


Fig. 28. Discrimination Time. Experiment *B*. Squad I. (Control.)

good as those on control days. Examination of the individual records, which have been platted out in the same way as the squad averages here shown, confirms these results throughout. No individual shows any clear indication of caffein influence.

In the case of Squad II. the caffein days are marked by great irregularity, but this is present before as well as after the dose, and is due chiefly to the variable records of Subject No. 11. This irregularity renders any inference uncertain. The control day yields a fairly uniform set of records. About all that can be said of this squad is that in the trials before the dose the reactions on caffein days happen to be slower than those on the control day, during the corresponding period, but that in the evening, when all three curves settle down to uniform levels, the caffein days are in no way inferior to the control day. There is here a bare suggestion that some 3–5 hours after the dose the result of the caffein is to counterbalance the

inferiority with which the days began, bringing the initially higher caffein curves to the same level as that of the initially superior control day. That is to say, in so far as the curves show any caffein influence at all, the indications are of stimulation some 3–5 hours after the dose. Any earlier effect, if present, is obscured by the original irregularity of the records. Examination of the individual records of the subjects throws no new light on the matter.

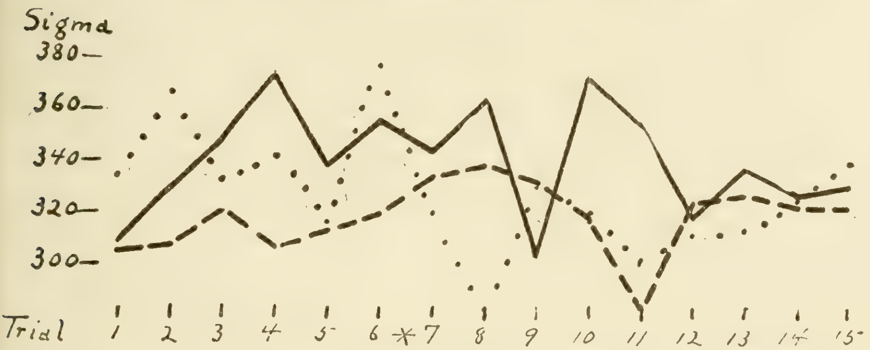


FIG. 29. DISCRIMINATION TIME. Experiment *B*. Squad II. (Solution.)

The curves for Squad III. follow a very regular course. On the first day (3 gr. caffein after 6th trial) the curve rises after the dose to a level some 20 sigma slower than that maintained before the dose, suggesting initial retardation. This effect, as it shows in the individual records, is clearly present with subjects 6, 8, 12 and 16, and is

FIG. 30. DISCRIMINATION TIME. Experiment *B*. Squad III. (3 gr.)

more pronounced and persistent in the case of 6, a woman. After 4.5 hours the curve falls to a level quite equal to that of the pre-caffein period. This is true for all the individuals except Subject 6, while Subject 12 shows considerable absolute stimulation as compared with

the morning performance. On the morning after the caffein day the curve runs constantly lower than on the preceding day, until about the time of the dose. This is true for all the individuals except Subject 6 in whose case the records of the second morning are not so good. For subjects 8, 9 and 12 the superiority is considerable, averaging about 30 sigma. From this point on the curve rises gradually, showing the normal slight fatigue effect already seen in the case of Squads I. and II. This is true of the individual records of all five subjects in the squad. There is no evidence of the apparent stimulation of the preceding day after the 12th trial. These late records are the poorest of the day. On the last day (again a control day) the curve shows little change throughout its course. Except for two slow reactions during the early trials the whole curve follows a uniform level. This is true for the individual curves of all but sub-

FIG. 31. DISCRIMINATION TIME. Experiment *B*. Squad IV. (6 gr.)

ject 8, in whose case the curve drops in the latter part of the day. If there is any caffein effect on this squad it is but slight, and that effect would seem to be an initial retardation followed by stimulation. There is certain evidence suggesting that this stimulation is still present on the following day. The evidence in this case is slight, however, and similar facts (the relative levels of the various morning curves) might lead one to infer retardation on the second morning after caffein, 45 hours after the dose. This result would, of course, seem quite improbable. But the persistence of caffein influence over so great an interval as 24 hours is again found in the case of Squad IV., was also suggested in the results of Experiment *A*, and was clearly found in the case of the Calculation test. There can be little doubt that it is a genuine effect.

The curves for Squad IV. resemble in every respect those of Squad III. The control curves, March 4 and 5, show the familiar slight tendency to fatigue as the day goes on. All three subjects agree in this respect. But on the caffein day the part of the curve

representing the last three hours sweeps consistently downward, as did that of Squad III. This again is true for all three subjects. As in the case of Squad III. again, this stimulation is apparently present on the following morning, the reactions there being the quickest made during the experiment. This is strikingly true with subject 14 and quite clearly so with subject 10. For the third subject, No. 3, the difference does not appear. As in the case of the third squad, the exception is the woman. If this is a genuine persistence of stimulation the low level of the curve at this time will of course have something to do with the general fatigue effect shown on this control day. The normal amount of fatigue will be overestimated somewhat and will appear more accurately on the 3d day, where it is, as a matter of fact, somewhat less conspicuous for all three subjects. There is no evidence of secondary depression following this stimulation. The subsequent trials on this second day are in no way inferior to those of the other control day. This squad took their dose (6 gr.) after the 6th trial on the first day.

The results of Experiment *B* are then thoroughly consistent with those of Experiment *A*. In the latter experiment, the retardation which was so clearly present, was produced by the smaller doses only. The initial retardation after 3 gr. doses was very slight. After 6 gr. doses there is no indication of retardation whatever, but on the contrary a very slight tendency to stimulation or else no effect whatever. When the stimulation was present it was found persisting on the following day, as was the retardation from the 1 and 2 gr. doses.

Experiment *C*

The records for Experiment *C* are given in Table LXXIX. In this experiment the dose was given in syrup form, only 15 minutes before the trial. But Experiments *A* and *B* agree in showing no effect on the discrimination reaction time until several hours after the dose. Quite in harmony with this result is the fact that in Experiment *C* there is no clear difference shown between the three types of days, the blank days, the plain syrup days and the caffeinated syrup days. The table presents the results for each subject, the figure meaning in each case the ratio between the average performance after the dose to the average performance before the dose. The table also gives the squad averages for each day, as well as for the various types of day. There is evidence of a very slight amount of sensory stimulation (not over 1 per cent.), and in this respect the caffein days differ not at all from the plain syrup days. As has already been pointed out in the case of some of the other tests, sen-

sory stimulation affects motor performance much more than mental performance, and since the caffein effect on mental performance is also slower than that on motor processes, Experiment *C* fails to yield any result whatever.

With the idea of investigating the effect of caffein on the accuracy and constancy of the process of discrimination involved in this test, record was kept of the number of false reactions on the part of each subject—reactions on the "blue key" when the stimulus turned

TABLE LXXIX

DISCRIMINATION. EXPERIMENT *C*

Giving for each subject the ratio of the average performance after the dose to the average performance before the dose. Over 100 means fatigue

Subject	1	3	4	7	8	9	10	11	12	13	14	15	Average
Blank	103.1	94.8	101.2	104.2	99.1	95.4	98.4	98.2	94.9	94.9	98.7	102.8	98.8
days	101.2	100.8	105.7	116.7	107.9	102.5	98.6	103.8	absent	100.7	104.4	87.7	102.7
												Av.	100.7
Syrup	107.9	102.4	98.2	88.7	107.8	103.3	92.1	100.5	100.9	96.9	91.4	93.2	98.5
days	98.1	96.2	101.6	96.1	107.1	102.8	95.8	94.0	110.7	110.7	102.1	95.9	100.5
												Av.	99.5
1.2 gr. caf.	96.6	95.3	102.3	100.9	93.6	98.3	98.0	94.4	87.3	92.1	104.0	94.8	96.4
3.6 gr. caf.	114.7	101.2	109.1	103.0	101.7	95.1	100.3	80.6	100.5	100.6	100.9	123.7	103.0
6 gr. caf.	101.5	98.0	97.7	87.9	97.7	103.3	98.3	97.0	absent	94.4	106.3	102.9	98.7
												Av.	99.4

out to be really red and reactions on the "red key" to the blue stimulus. Table LXXX. gives a summary of these records in Experiment *A*. The number of false reactions was very small in the case of all the subjects. The table gives the total number of false reactions during the trials before the dose and the total number during the trials after the dose, for each subject, on both control and caffein days, with no attempt to separate the data according to the size of the dose, because of the small number of false reactions during the month. The table gives also the ratio of false reactions after the dose to the number before the dose, for both sugar and caffein. The squad records are also computed, using the total number of false reactions made by the squad, instead of the average. Squad I. shows no difference between the real control days and the pseudo-caffein days. The average ratio of errors after the dose to errors before the dose being 63 per cent. on one type of day and 62 per cent. on the other. In the case of Squad II. (dose in forenoon) the caffein days are for all three subjects superior (after the dose) to the control days. It will be recalled that by one method of com-

TABLE LXXX

Discrimination. Experiment *A*

Giving the total number of false reactions made by each subject during Experiment *A*. In the first column under a given type of day are given the total number of false reactions in the trials preceding the dose. In the second column occur the total number in the trials after the dose, for both control and caffein days. In the third column is given the ratio of the number of false reactions after the dose to that before. The subjects are assembled by squads according to character and time of the dose.

	Control Days			Caffein Days		
Subject	Before	After	Per Cent.	Before	After	Per Cent.
1	9	7	67.7	16	8	50.0
4	14	7	50.0	11	7	63.5
7	16	15	93.5	15	7	46.6
15	8	1	12.5	5	7	140.0
Squad I.—Total	47	30	63.0	47	29	62.0
8	9	21	231.0	14	10	71.4
12	17	35	203.0	16	26	162.0
13	15	19	126.5	19	24	126.0
Squad II.—Total	41	75	183.0	49	60	122.0
3	21	7	33.3	17	17	100.0
9	17	13	76.5	22	9	41.0
14	33	11	33.3	24	17	70.6
Squad III.—Total	71	31	44.0	63	53	84.0
5	10	6	60.0	14	9	64.0
6	16	4	25.0	6	3	50.0
10	17	7	41.2	16	11	68.7
11	31	21	67.7	33	28	85.0
16	23	13	56.5	27	15	55.7
Squad IV.—Total	97	51	53.0	96	66	69.0

putation this squad showed retardation for small doses and stimulation for large, while by another method it showed stimulation for all doses at the earlier periods and retardation at a later period for medium doses only, the small and large amounts appearing to produce stimulation. Our conclusion was that no consistent effect could be made out on this squad. It is at least clear that the caffein did not produce increased number of errors. Squad III., on the other hand, showed, by both methods of computation, retardation for small doses. And here the number of mistakes on the caffein days (after the dose) is seen to be greater by 40 per cent. than the number made on control days. Squad IV., with similar caffein effect from the point of view of time, shows a caffein inferiority of 16 per cent. Only one subject out of the eight in these two squads is an exception to this rule.

Table LXXXI. shows the number of false reactions at each trial on each of the three intensive days of Experiment *B*. The upper part of the table gives the total number of false reactions made at each of the 15 trials by the 10 subjects comprising the various caffein squads. The lower part gives similar figures for the control squad. This table gives a good measure of the accuracy with which these discriminations were made, indicating the complete absence of guesses in the reactions. Out of a total of between 8,575 and 11,700 reactions there were only 390 false reactions, about 4 out of every 100. But caffein seems to have had no influence whatever on the number of these false reactions. Whether the records after the dose are compared with those before the dose, or whether the caffein days are compared with control days, no difference whatever is found.

TABLE LXXXI

FALSE REACTIONS. EXPERIMENT *B*

The upper part of the table gives the total number of false reactions made by members of the caffein squads, and the lower part those by the control squad, at each trial on each of the three days. Dose after the sixth trial.

Caffein squad 10 subjects ; 150 to 200 reactions at each trial

Trial	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Mar. 3	18	12	8	6	4	9	12	8	6	6	6	11	7	10	5
Mar. 4	7	6	10	4	4	7	9	13	6	6	7	5	7	11	9
Mar. 5	7	10	8	4	7	4	8	5	9	4	9	5	6	7	8

Control squad 3 subjects; 45 to 60 reactions at each trial

Trial	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Mar. 3	0	2	1	0	1	2	1	2	2	2	1	1	1	0	0
Mar. 4	0	4	0	2	0	1	0	2	8	0	0	1	0	0	0
Mar. 5	0	2	3	0	2	1	2	1	0	1	1	1	2	1	0

The significance of these facts is at once apparent. In Experiment *A* the squads showing retardation showed also a greater per cent. of false reactions. The squad showing no such retardation clearly, showed no such increase in false reactions, but rather a decrease. In Experiment *B*, in which there is no clear evidence of retardation the number of false reactions is found to be unaffected. To those familiar with the conditions of the discrimination reaction experiment, or to any one observing the behavior of subjects under the influence of caffein at such a test, it will at once occur that the retardation found in the discrimination times is in all probability an effect of stimulation. In such a test stimulation is likely to lead to a fatal briskness in the first few reactions in a series, resulting in false reactions and in a subsequent voluntary caution as a measure designed to counteract these false reactions. Such an effect would

appear in the records as a retardation in discrimination time. The retardation consequent upon small doses and the stimulation produced by larger amounts, in this test, does not then point to any complexity in the character of the caffein action. It seems much more likely that the effect is really stimulation for all doses, the amount perhaps increasing with the size of the dose. In the case of large amounts in the present experiment the great amount of stimulation produced combined with the fact that by the time these doses were given the subjects had had almost a month's practise in the

TABLE LXXXII

VARIABILITY. EXPERIMENT *B*

Giving the average variability of the individuals from their own average in the 10 reactions of a given sitting. The table gives the squad averages for each trial on each of the three days. These M.V.'s are seen to be about 10 per cent. of the discrimination time. Dose after the sixth trial. Trials given consecutively from 1 to 15.

							Squad I								
Mar. 3	26	37	44	40	38	32	33	39	28	44	44	34	37	47	38
Mar. 4	30	30	53	36	49	39	40	33	46	59	45	48	53	39	40
Mar. 5	32	39	40	52	44	47	52	46	40	57	40	52	50	40	38
							Squad II								
Mar. 3	29	36	51	38	42	37	50	38	29	33	46	34	58	42	47
Mar. 4	48	35	35	44	40	52	52	73	50	54	49	32	60	36	46
Mar. 5	36	42	24	41	60	64	45	51	43	66	34	46	54	41	29
							Squad III								
Mar. 3	22	27	28	27	32	27	28	36	33	37	33	24	35	36	32
Mar. 4	28	40	31	34	34	37	40	30	41	37	42	43	37	46	42
Mar. 5	34	42	38	32	45	38	33	43	39	38	36	48	36	31	29
							Squad IV								
Mar. 3	20	17	24	35	39	33	42	24	39	41	36	25	29	29	34
Mar. 4	34	33	26	31	28	27	29	33	31	32	23	27	53	29	28
Mar. 5	32	32	36	33	50	28	36	27	39	35	33	32	30	37	25

test really led to accelerated reaction times. A similar explanation no doubt is at the bottom of the retardation produced by large doses in the case of the three-hole (coordination) test and the typewriting test.

Up to this point nothing has been said of the variabilities of these discrimination times. It has not seemed worth while to give these variabilities in full, because of the large amount of space that would be required. But in order to give some notion of their magnitude Table LXXXII. has been compiled. Each record in this table is the average of the M.V.'s of all the subjects in a given squad at a

stated test. The 10 trials on the part of each individual were averaged. The M.V. of this individual average was computed. This was done for each individual in the squad. The average of these personal M.V.'s, at each test on each of the three intensive days, is the record to be found in Table LXXXII. Since the discrimination times range between 300 sigma and 400 sigma, the M.V.'s are in the long run about 10 per cent. of the discrimination time. This per cent. will serve as an approximate statement of the variability of the averages discussed in the preceding paragraphs of this chapter. The variability does not seem to be in any way influenced by the caffein doses. The general tendency is for the variability to increase somewhat as the day goes on, but this tendency is not always present, nor is it found with all individuals, though it shows up fairly well in the averages. This increase begins at about the time of the 6th trial, after which the dose was always taken. But the caffein days and control days show no clear difference either in the amount or in the time of appearance of this increase.

Summary

Small amounts of caffein tend to produce retardation in discrimination time, this retardation being accompanied by a greater number of false reactions. The false reactions appear to be caused by a preliminary briskness produced by the caffein, and the retardation in reaction time caused by a voluntary caution in the attempt to eliminate the false reactions. This is a test in which stimulation does not make for efficiency except after long practise. Larger amounts of caffein produce within 2 hours after the dose a stimulation so great that the retardation following small doses does not appear. Greater familiarity with the test may also contribute to this effect. The caffein effect seems very persistent in this test, and traces of it are found on the following day. When retardation is present it does not appear until very late, whereas the stimulation comes quickly. This again appears to be due to the size of the dose, the larger amounts acting more quickly. The caffein does not seem to modify the variabilities of the reaction times. When the dose is taken in the morning no effect can be consistently made out. When it is taken along with the mid-day meal the retardation for small doses tends to disappear. The magnitude of the caffein influence varies inversely with body weight, as in most of the other tests.

CHAPTER XII

The Influence of Caffein on the Cancellation Test

One unsatisfactory feature of the use of simple tests is the fact that it is not always apparent what function or set of functions, what process, faculty or capacity is being measured. Thus the cancellation test has been used by various investigators for the measurement of such varying factors as "degree of attention," "discrimination," "rate of perception," "fatigue," "capacity to break and form associations," "distraction," etc. One would suppose that certain of the numerous tests devised for experimental purposes would be more or less closely related to each other; not necessarily to the degree of assuming particular "faculties" involved in their performance, but at least to the degree that similar nervous mechanisms might be employed. Such tests would theoretically correlate with each other from the point of view of individual proficiency. But the method of correlating proficiencies has so far failed to afford a reliable classification of the various tests. The difficulty perhaps arises from the fact that relative performance in specific tests is largely conditioned by unequal and more or less particularized skill, interests, familiarity and adaptation with respect to operations somewhat analogous to those involved in certain of the tests. Consequently correlations in performance would not reflect qualitative resemblances and differences in the tests, but rather the presence or absence of these various sorts of specialization. As an illustration, the cancellation test might involve the same functions as the color discrimination reaction test, and yet the two tests not correlate to any marked degree in a given individual because of specialized skill in proof reading, or because of individual differences in the method of performing the test.

Conceivably similarity of function or of nervous mechanism involved might be determined by comparing the qualitative or quantitative effect, on the two tests, of an influence which is more or less specialized and localizable in its action on nervous process, such an influence as that of a drug with nervous action. The application of this criterion the writer believes to be possible. Such application seems to throw new light on the character of the hitherto vaguely classified cancellation test.

This test was one of the three which were omitted in the three-

TABLE LXXXIII

Cancellation Test. Squad I., Experiment *A*. Daily Averages

	February																		
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
7:45	95.6	94.4	96.1	97.7	95.3	93.4	88.7	92.5	90.5	90.5	95.5	88.8	97.3	96.1	97.4	90.3	83.8	83.7	90.7
10:00	94.5	83.5	90.2	95.9	87.9	92.3	93.7	95.3	92.4	100.9	88.7	94.7	93.9	89.1	93.0	91.9	90.1	86.5	90.5
12:00	97.3	101.2	99.1	100.5	102.5	99.2	98.3	96.3	97.4	92.9	91.5	97.6	90.7	83.9	90.7	89.2	81.4	88.9	89.2
1:00	Dose, sugar capsules only, daily at 1:00 P.M.																		
3:10	108.3	105.7	103.2	116.9	107.5	117.4	104.7	110.9	110.5	115.1	111.3	106.3	105.9	105.2	99.5	97.3	102.7	98.7	104.0
5:30	95.1	96.9	97.1	93.8	95.8	91.8	91.8	91.8	87.2	85.3	90.3	89.6	85.9	92.9	90.3	81.6	84.2	88.9	85.0

TABLE LXXXIV

Cancellation Test. Squad II., Experiment *A*

	February																		March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	101.8	98.6	*91.7*	*94.7*	*98.6*	90.2	87.1	*96.5*	*89.9*	*89.2*	92.3	84.3	86.6	*87.3*	*78.3*	*83.7*	81.6	79.8	81.9	*79.4*	75.5
10:00	98.9	101.3	*102.2*	*100.8*	*101.9*	94.3	95.7	*97.9*	*99.4*	*98.1*	89.1	100.9	84.0	*94.9*	*88.5*	*84.4*	91.9	83.2	82.7	*89.5*	79.3
10:30	S	S	*1 gr.*	*1 gr.*	*1 gr.*	S	S	*2 gr.*	*2 gr.*	*2 gr.*	S	S	S	*4 gr.*	*4 gr.*	*4 gr.*	S	S	S	*6 gr.*	S
12:00	96.4	98.7	*93.0*	*91.9*	*91.1*	91.5	91.7	*82.7*	*86.2*	*84.3*	88.2	81.7	88.5	*79.1*	*82.7*	*80.7*	86.3	78.9	80.3	*80.2*	83.8
3:10	116.5	103.3	*109.3*	*99.9*	*110.5*	109.7	108.9	*101.1*	*101.9*	*101.2*	98.9	97.9	104.3	*101.4*	*88.5*	*96.7*	97.3	94.6	95.8	*91.6*	94.8
5:30	100.1	99.3	*102.3*	*98.0*	*90.5*	84.9	86.0	*82.1*	*82.3*	*81.7*	82.3	78.1	78.9	*75.3*	*73.4*	*76.6*	79.6	74.1	78.4	*70.2*	73.5

TABLE LXXXV

Cancellation Test. Squad III., Experiment *A*

	February																		March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	114.0	*106.5*	103.0	*96.1*	94.8	*88.4*	96.5	*86.8*	100.9	*87.2*	85.1	*87.6*	92.6	*94.9*	84.2	*81.8*	86.1	*92.0*	88.2	*83.1*	80.7
10:00	93.3	*86.0*	102.6	*83.4*	88.5	*94.7*	90.2	*94.1*	94.9	*95.1*	85.0	*87.8*	81.5	*82.1*	83.9	*75.0*	82.0	*79.9*	80.9	*81.6*	78.8
12:00	92.9	*89.5*	89.8	*100.4*	84.4	*90.1*	92.9	*92.9*	92.9	*99.1*	87.3	*89.9*	84.0	*85.6*	89.2	*86.6*	79.0	*83.6*	81.0	*84.2*	82.5
1:00	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	110.0	*109.4*	109.1	*111.5*	112.5	*119.0*	113.5	*113.3*	100.7	*102.0*	94.2	*100.9*	101.1	*87.1*	102.6	*91.6*	87.6	*94.0*	97.2	*91.1*	90.3
5:30	89.7	*86.6*	88.5	*78.5*	83.9	*81.3*	89.5	*90.8*	77.9	*75.1*	77.9	*72.3*	74.0	*77.5*	80.3	*76.2*	77.6	*70.9*	76.0	*74.8*	78.1

TABLE LXXXVI

Cancellation Test. Squad IV., Experiment *A*

	February																		March	
	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	1	2
7:45	110.6	*103.5*	101.3	*100.2*	99.2	*92.5*	101.9	*96.4*	93.8	*95.5*	99.0	*88.7*	89.6	*85.5*	84.3	*89.1*	83.0	*92.0*	87.0	*91.2*	95.4
10:00	102.2	*99.8*	97.1	*95.4*	99.4	*98.6*	96.9	*91.3*	96.7	*93.3*	94.5	*90.4*	88.6	*86.4*	84.9	*86.6*	80.3	*88.4*	88.7	*91.3*	93.3
12:00	98.2	*93.5*	92.3	*93.7*	95.4	*92.5*	97.5	*96.5*	94.3	*97.3*	90.0	*87.7*	88.4	*86.6*	88.0	*91.0*	79.2	*92.4*	85.6	*82.0*	81.7
1:45	S	*1 gr.*	S	*1 gr.*	S	*2 gr.*	S	*2 gr.*	S	*3 gr.*	S	*3 gr.*	S	*4 gr.*	S	*4 gr.*	S	*6 gr.*	S	*6 gr.*	S
3:10	124.1	*106.5*	112.4	*114.0*	112.0	*119.3*	112.8	*111.3*	108.0	*113.7*	105.7	*105.9*	113.0	*110.7*	104.9	*105.0*	103.3	*99.4*	96.5	*99.4*	101.9
5:30	94.6	*89.6*	96.0	*94.1*	101.4	*89.6*	90.6	*86.4*	86.1	*87.6*	86.1	*91.0*	86.8	*81.6*	92.9	*83.0*	82.2	*83.7*	83.8	*85.2*	84.2

day intensive experiment (*B*) and in the seven-day experiment with syrups (*C*). Hence the only records available are those secured in Experiment *A*. The test is an unsatisfactory one to use, in the first place because of the impossibility of maintaining uniform illumination except by artificial means. This provision was not adequately controlled in Experiment *A*. In the second place, since different characters were cancelled (2, 3, 5, 6, 8) at each of the five tests on a given day and since these characters differ among themselves in perceptibility, the successive trials on a given day can not satisfactorily be compared with each other. The comparison of similar tests on different days is not influenced by differences in the character cancelled, but such comparisons, under only roughly controlled conditions of illumination are uncertain in value. The individual curves are for the most part very irregular and the squad records are correspondingly so. The conclusions as to the influence of caffein on the cancellation test are consequently given with considerable reservation.

Tables LXXXVII.–LIX. give the comparison of actual with calculated records for Squads I., III. and IV. Table LXXXVIII. gives the averages for each trial on each type of day, for Squad II., the average of the two trials before the dose being adopted as the standard for the day and the ratios for the later trials referring to this standard.

At the 3:10 test the control squad (I.) shows a tendency to be slow in pseudo-caffein days. Squad III. shows the same tendency except for the larger doses, which yield stimulation. Squad IV. gives balanced records, differing from the control squad only in the absence of retardation. At the 5:30 test the control squad yields records which balance. Squad III. shows a small amount of stimulation, as does Squad IV. for the smaller doses. On the post-caffein days (morning records) the control squad yields balanced records at all hours. Squad III. at the 7:45 and Squad IV. at all three morning trials suggest retardation after the small and continued stimulation after the large doses of the preceding caffein days.

Squad II. (Table LXXXVIII.) after 1 gr. yields records not distinguishable from those of the control days. But after 2, 4 and 6 gr. all the subsequent trials show stimulation, which is larger at the 12:00 test for the 2 gr. dose, but greatest for the 6 gr. amount at the later 5:30 trial. The average per cent. stimulation is greatest in the case of the 2 gr. dose.

In Tables XCI.–XCII. the records for this Squad II. are again computed, the individuals being treated separately, the 12:00 and 3:10 trials being averaged for one table and the 5:30 trial being

TABLE LXXXVII

CANCELLATION. SQUAD I., EXPERIMENT A

Sug. Av.	7:45 Caf. Av.	Dif.	Sug. Av.	10:00 Caf. Av.	Dif.	Sug. Av.	12:00 Caf. Av.	Dif.	1:00 Dose	Sug. Av.	3:10 Caf. Av.	Dif.	Sug? Av.	5:30 Caf. Av.	Dif.
95.8	96.0	— 0.2	90.7	89.7	+ 1.0	99.5	100.9	— 1.4	S	105.6	111.3	— 5.7	96.3	95.4	— 0.9
90.8	93.0	— 2.2	91.9	93.8	— 1.9	99.1	97.8	+ 1.3	S	106.4	114.3	— 7.9	91.7	91.8	+ 0.1
94.7	89.6	+ 5.1	90.9	97.8	— 6.9	92.6	94.7	— 2.1	S	109.8	110.7	— 0.9	88.4	87.4	— 1.0
94.2	93.2	+ 1.0	92.5	90.5	+ 2.0	88.4	86.5	+ 1.9	S	101.9	101.2	+ 0.7	87.7	87.3	— 0.4

TABLE LXXXVIII

CANCELLATION. SQUAD II., EXPERIMENT A

Dose	Av. 1st and 2d Trials	3d Trial	4th Trial	5th Trial	Av. 3, 4 and 5
Sugar Av.	91.4	89.2	103.8	86.0	
Per cent. of Av.	100.0	97.7	113.4	94.3	101.8
1 gr. caffein	95.7	94.8	110.4	90.1	
Per cent. of Av.	100.0	99.0	115.0	94.0	102.6
2 gr. caffein	95.0	84.1	101.4	82.0	
Per cent. of Av.	100.0	88.3	106.7	86.3	93.7
4 gr. caffein	86.0	80.8	95.5	75.1	
Per cent. of Av.	100.0	94.0	111.0	87.2	97.4
6 gr. caffein	84.5	80.2	91.6	70.2	
Per cent. of Av.	100.0	95.0	107.3	83.0	95.1

TABLE LXXXIX

Cancellation. Squad III., Experiment *A*

7:45 Sug. Av.	7:45 Caf. Av.	Dif.	10:00 Sug. Av.	10:00 Caf. Av.	Dif.	12:00 Sug. Av.	12:00 Caf. Av.	Dif.	1:00 Dose	3:10 Sug. Av.	3:10 Caf. Av.	Dif.	5:30 Sug. Av.	5:30 Caf. Av.	Dif.
103.7	101.3	+ 2.4	96.5	84.7	+11.8	88.7	95.0	— 6.3	1 gr.	110.2	110.4	— 0.2	87.7	82.5	+ 5.2
97.2	87.6	+ 9.6	90.9	94.4	— 3.5	90.8	91.5	— 0.7	2 gr.	109.8	116.1	— 6.3	85.2	86.0	— 0.8
90.4	87.4	+ 3.0	86.6	91.4	— 4.8	87.9	94.5	— 6.6	3 gr.	97.6	101.4	— 3.8	77.0	73.7	+ 4.0
86.8	88.3	— 1.5	82.8	78.5	+ 4.3	85.4	86.1	— 0.7	4 gr.	98.5	89.4	+ 9.1	78.1	76.9	+ 1.2
85.8	87.6	— 1.8	80.6	80.8	— 0.2	80.8	83.9	— 3.1	6 gr.	93.1	92.5	+ 2.4	76.9	72.4	+ 4.5

TABLE XC

Cancellation. Squad IV., Experiment *A*

7:45 Sug. Av.	7:45 Caf. Av.	Dif.	10:00 Sug. Av.	10:00 Caf. Av	Dif.	12:00 Sug. Av.	12:00 Caf. Av.	Dif.	1:45 Dose	3:10 Sug. Av.	3:10 Caf. Av.	Dif.	5:30 Sug. Av.	5:30 Caf. Av.	Dif.
103.1	101.8	+ 1.3	98.9	97.6	+ 1.3	94.7	93.1	+ 1.6	1 gr.	110.2	110.2	0.0	97.0	91.9	+ 5.1
99.2	94.5	+ 4.7	97.5	94.9	+ 2.6	96.2	94.5	+ 1.7	2 gr.	111.4	115.3	— 3.9	92.2	88.0	+ 4.2
95.4	92.1	+ 3.3	93.6	92.9	+ 0.7	100.7	92.5	+ 8.2	3 gr.	108.1	108.8	— 0.7	86.3	89.3	— 3.0
85.3	87.8	— 2.5	84.7	86.5	— 1.8	85.9	88.8	— 2.9	4 gr.	106.5	107.8	— 1.3	86.2	82.3	+ 3.9
88.1	91.6	— 3.5	87.6	89.9	— 2.3	83.0	87.2	— 4.2	6 gr.	99.6	99.4	+ 0.2	83.5	84.4	— 0.9

presented separately. The ratios are presented for each individual, and the squad average in these tables is therefore not the ratio of the average record of the squad but the average of the ratios of the three individuals. Hence the ratios do not completely correspond to those given in Table LXXXVIII.

The ratios in Table XCI. can not be compared directly with those in Table XCII., since at these various trials different digits were cancelled and the difference between the two tables is chiefly a function of the difference in the relative ease of marking out these different characters. But within any one table the control days may be compared with the caffein days. If this is done all doses of caffein are seen to have produced stimulation at the 12:00 and 3:10 tests (dose at 10:30), only one of the caffein ratios (subject 12 for 1 gr.) being larger than the corresponding control ratio. But by the time of the 5:30 test this 1 gr. dose has produced retardation with all three subjects, the average amount being almost 5 per cent. All larger doses yield stimulation for all three subjects. On the basis of these tables combined with the evidence from Table LXXXVIII., the caffein effect on Squad II. is clearly a general tendency toward stimulation, except for the small doses, which produce records inferior to those of control days.

TABLE XCI

Cancellation. Squad II. Experiment *A*

Ratio of 12:00 and 3:10 trials (average) to trials before dose (av.)

Subj.	Control Average	M.V.	1 Gr.	M.V.	2 Gr.	M.V.	4 Gr.	M.V.	6 Gr.
8	1.078	.071	.965	.043	1.036	.073	1.039	.030	1.075
12	1.101	.057	1.144	.053	1.052	.066	1.093	.126	1.023
13	1.019	.047	.933	.077	.900	.063	.945	.027	.958
Av.	1.066	.058	1.014	.058	.996	.067	1.026	.061	1.018

TABLE XCII

Cancellation. Squad II. Experiment *A*

Ratios of 5:30 trials to trials before dose (av.)

Subj.	Control Average	M.V.	1 Gr.	M.V.	2 Gr.	M.V.	4 Gr.	M.V.	6 Gr.
8	.783	.048	.817	.040	.765	.063	.750	.030	.705
12	1.081	.039	1.130	.020	1.020	.036	1.001	.103	.955
13	.978	.065	1.036	.120	.854	.050	.906	.033	.882
Av.	.947	.051	.994	.060	.879	.049	.886	.055	.847

When the records for Squads I., III. and IV. are treated by this ratio method, the ratios of each subject being computed separately and the squad record consisting of the average of these ratios,

Tables XCIII. to XCV. result. The control squad (Table XCIII.) shows slightly inferior records on the pseudo-caffein days (about 2 per cent.). There are but three subjects in this squad for this test, subject 1 having been unable to complete the test on account of eyestrain. Of these three subjects two are better on control days and one on pseudo-caffein days.

In the case of Squad III. (Table XCIV.) all three subjects show retardation after the small doses (1 and 2 gr.) and stimulation after larger amounts. In the squad average this effect shows up clearly, the retardation from 1 and 2 gr. averaging 4.5 per cent. and the stimulation from larger amounts averaging 5.6 per cent. This squad thus quite confirms the conclusions based on the records of Squad II.

TABLE XCIII

CANCELLATION. SQUAD I., EXPERIMENT *A*

Ratios of performance after dose to performance before dose

Subj.	Control Av.	M.V.	Pseudo-Caf. Av.	M.V.
1	Test omitted because of eyestrain.			
4	1.061	.023	1.077	.047
7	1.096	.057	1.085	.061
15	1.021	.027	1.079	.067
Average	1.059	.036	1.080	.058

TABLE XCIV

CANCELLATION. SQUAD III., EXPERIMENT *A*

Ratios of performance after dose to performance before dose

Subj.	Control Average	M.V.	1 Gr.	2 Gr.	3 Gr.	4 Gr.	6 Gr.	Av. of 1 and 2 Gr. (Also Differences betw. These and Control Av.)	Av. of 3 to 6 Gr.
3	.961	.071	1.043	.993	.885	.852	.823		
			.910	.942	.908	.966	.963	.972	.899
			.976	.967	.896	.909	.893	+.011	—.062
9	1.099	.051	1.010	1.170	1.046	1.068	1.093		
			1.103	1.260	1.020	1.070	1.060	1.137	1.059
			1.056	1.218	1.033	1.069	1.076	+.036	—.040
14	1.029	.062	1.080	1.120	.888	.905	.990		
			1.040	1.220	.950	1.072	.972	1.115	.963
			1.060	1.170	.919	.988	.981	+.086	—.048
Av.	1.029	.061	1.030	1.118	.949	.988	.983	1.074	.973
Difference between caffein and control averages								+.045	—.056

In the case of Squad IV., taking the dose in mid-afternoon the retardation runs higher up along the scale of doses, and the five

subjects do not agree among themselves. No. 5 is retarded by all but the 2 and 3 gr. amounts, these giving stimulation. In the case of No. 6 it is just these two amounts that yield retardation, other doses producing apparent stimulation. Subjects 10, 11 and 16 show no uniform tendency except that all are stimulated by the 6 gr. amount.

TABLE XCV

CANCELLATION. SQUAD IV., EXPERIMENT *A*

Ratios of performance after dose to performance before dose

Subj.	Control Average	M.V.	1 Gr.	2 Gr.	3 Gr.	4 Gr.	Av. of 1 to 4 Gr. Also Difference Between These and Control Ave.	6 Gr.
			1.035	1.055	1.075	1.190		1.050
			1.160	1.020	1.017	1.117		1.180
5	1.085	.070	1.097	1.037	1.046	1.153	1.083	1.115
							—.002	+.030
			1.050	1.072	1.030	1.059		.915
			.977	1.000	1.130	.943		.970
6	1.021	.077	1.013	1.036	1.080	1.001	1.032	.942
							+.011	—.079
			1.225	1.100	1.025	1.100		1.052
			1.050	.995	1.150	1.093		1.080
10	1.093	.057	1.137	1.047	1.082	1.096	1.090	1.066
							—.003	—.027
			.853	1.030	1.015	1.034		.956
			1.053	1.025	1.000	1.047		.950
11	1.037	.095	.953	1.027	1.007	1.040	1.007	.953
							—.030	—.084
			1.061	1.270	1.150	1.200		1.050
			1.161	1.222	1.285	1.108		1.110
16	1.131	.086	1.111	1.246	1.217	1.154	1.182	1.080
							+.051	—.051
Av.	1.073	.077	1.060	1.078	1.086	1.088	1.078	1.031
							+.005	—.042

In the squad averages this situation leads to the same result as that found with the other two squads. Up to 4 gr. there is very slight retardation (the average amount being .5 per cent), while the large dose yields 4.2 per cent. stimulation.

The general tendency must be said to be stimulation, except for small doses, and for larger doses in the case of some subjects. In the introspective reports of the subjects it is quite generally stated

that difficulty in this test arose from the tendency to skip digits and from the consequent necessity of going back and hunting for them (there were five digits in each line and the assistant called out each time a digit in a line had been omitted, the subject being required to mark out all digits before his record was complete). It looks as though the retardation might have come from a real stimulation and a consequent briskness which led to omissions in the attempt to do the test quickly.

It appears further that individual differences in this test and in the character of the caffein effect may depend largely on individual ways of performing the task. With some subjects the test seems to involve simply a recognition process, without any considerable amount of discrimination. Thus subject 12 says of this test, "In the course of the experiment I tried to allow my eye to run along in advance of my pencil, without identifying any number except those I wanted to cancel." After the first 4 days of the caffein weeks this subject shows no retardation. No. 14 also says, "As the experiment progressed I tried to make the eye take in about one third of a line at a time and do it rapidly, just trusting that somehow I would not overlook one." After the first few days of the experiment this subject got an average stimulation of 5 per cent. from the caffein doses. Subject 9, on the other hand, writes, "At the beginning of the experiment the figures which I was cancelling seemed to stand out from the rest. This, after a few days, disappeared and I had to scan every figure in order to get all of those on which I was working." This subject's record shows that during the first part of the experiment, in which the simple method of recognition seems to have been followed, the caffein (1 gr.) produced a stimulation of 4.3 per cent. But from that point on the 2 gr. amounts produced retardation and after the 3, 4 and 6 gr. doses the ratio of the 1 gr. was never equalled. That is to say, in some cases this cancellation test seems to be carried on in terms of simple recognition, direct perception and motor response, with little discrimination involved. In other cases it seems necessary to perform a real act of discrimination in which the correct figure is not merely recognized in itself but more or less explicitly distinguished from the others. In the former case the caffein influence tends to be stimulation without any interference. In the latter case the test resembles the discrimination-reaction experiment, and during the early part of the experiment the effect of the caffein is disastrous because the briskness of performance results in omissions and consequent delay in recovery.

Comparison of these results with the character of the caffein influence in the case of the other tests is interesting from the point of

view of the attempt at qualitative classification suggested in an earlier paragraph. In the case of the tapping test there was stimulation also, but this effect came quickly and increased directly with the size of the dose. In the coordination and typewriting tests small amounts produced stimulation but large amounts retardation. Calculation, opposites and color-naming showed stimulation for the total range of doses. Discrimination time was retarded by small and quickened by large amounts. The cancellation test does not clearly resemble any of these groups. On the basis of the squad averages and of some of the individual records it resembles the discrimination time test. But some of the subjects get effects much more similar to those produced on tests involving processes of direct recognition and simple association. These results show the cancellation test to be just such an indefinite and borderline test as the earlier description of it implied. The process measured by it will vary with the individual and at different times in the history of the same individual's performance. Attempts to correlate proficiency in the cancellation tests with other sorts of ability have usually resulted in very low correlation. This may perhaps be partially explained by the indication that the test does not measure the same process, function or capacity under different circumstances or with different individuals.

Summary

The caffein influence on the cancellation test does not usually show itself until several hours after the dose. Perhaps as a result of this tardiness the effect seems to be greater in the case of the squad taking the dose in the morning. The general effect is stimulation for large doses, but small doses show retardation in the squad averages, while some individuals get retardation for larger amounts. The results are influenced considerably by individual differences in the method of performance. When the test seems to proceed in terms of simple recognition of the figure to be cancelled, the result is stimulation, as in the tests of association processes and speed of movement. But when it is necessary to examine each figure on the sheet, discriminating in a rather explicit way the correct from the false characters, the test comes to resemble the discrimination reaction in character and in the nature of the caffein influence. In the absence of intensive tests nothing can be said concerning the persistence time of this influence and nothing with assurance concerning its after effect. There is a bare indication of continued retardation in the records on mornings after caffein days on which small doses were given. The results do not recommend the test for purposes similar to those of the present investigation.

CHAPTER XIII

The Influence of Caffein on Sleep[1]

Throughout the experiment each subject kept daily records of his general condition of health and spirits throughout the day, indicating in his special "Daily Health Book" any signs of bodily distress or discomfort, such special or general organic, digestive and nervous disturbances as might be noted from time to time, and in general as good an introspective account as possible of his mood and tonus. In addition to stating the character of any symptoms or unusual observations, the time of their appearance and their duration was noted and reference made to any outside factor which might have been responsible for the condition described. This health record was divided into two parts, the first having to do with the day-time hours preceding the evening on which the entry was made, and the second having to do with the night-hours following. This second entry was made on the following morning, immediately on arrival at the laboratory. At this time each individual recorded the approximate number of hours which he had slept during the night and described the quality of his sleep as "better than usual," "ordinary," or "worse than usual." In working up these data the approximate number of hours was accepted as stated. From the point of view of character or quality an attempt was also made to express the effect of caffein in numerical form. Representing the individual's *usual* quality of sleep by a value of 2, letting the value 1 indicate sleep which according to the judgment of the individual himself was *better than usual,* and letting the value 3 represent the quality of sleep which the individual himself judged to be *worse than usual,* tables were compiled from the introspective records occurring in the daily health books. In the individual averages then as well as in the squad averages, a value of 2.00 indicates normal sleep, all values less than 2.00 indicate sleep judged unusually good while all values larger than 2.00 indicate impaired sleep.

It will be recognized at once that these measures of sleep quality are only rough and approximate, but it is just as obvious that they are as accurate measurements of the thing in question as can well be secured. The individual himself is the only one who knows any-

[1] Reprinted from *American Journal of Psychology,* January, 1912.

thing about his customary sleep quality. In daily life we pass such judgments as those given in the present experiment, and in much the same way, with about the same number of categories. We pronounce ourselves to have slept well or poorly or about as usual. Finer distinctions would probably have little or at most exceedingly variable meaning. With respect to the criteria on which such judgments are based there are also considerable individual differences. The number of hours slept through, the number and character of the dreams, the interval after retiring before sleep ensued, the number of times awakened during the night, general feelings of relief or languor after arising, etc., all play their part no doubt. And the judgment is at best only a secondary one, that is to say the quality of sleep is inferred from introspections and observations made during waking moments. In spite of this, few judgments of this subjective character would seem to have higher reliability than the individual's own opinion of the satisfactoriness of his slumber, and when the categories are limited to the three employed here, the judgments are delivered with a high degree of confidence. The reliability of the judgments is moreover emphasized by the consistent conclusions suggested by the various tables.

The statements of number of hours sleep are at best only approximate, in cases of impaired sleep. When the slumber was not disturbed the figures are more reliable, since all the subjects observed regular hours of retiring and arising. It will not be possible here to compare the individual subjects or the squad averages with each other, except in so far as the number of hours' sleep on control days is adopted as the normal in each case. A fairly constant average of about 7.5 hours sleep appears to be the normal for all of the subjects, as is indicated by the records for the week before the caffein doses began, and also by the records for the control days during the caffein experiments.

The records on sleep fall into two general sections, the first covering the 28 days of the caffein alkaloid experiments, and the second covering the 7 days of the experiments with syrups and carbonated water, with and without caffein contents. In the following tables the first column gives the averages for the first week, during which only sugar doses were given to all subjects. The second column gives the averages for the control days during the following three weeks. These two columns may then both be considered as normals. The third column gives the averages for the days on which the dose was either 1 or 2 gr. of caffein, the fourth column the records for the 3–4 grain doses, and the fifth column for the 6 gr. doses. The last column gives the final average for all the caffein days (1–6 gr. doses).

The individual subjects are grouped in squads according to the time at which the dose was taken and according to the distribution of the doses during the month.

Squad I., consisting of subjects 1, 4, 7, and 15, were the control squad, and ran throughout the four weeks on sugar doses only. Nevertheless their records have been averaged as though they had taken the caffein on alternating days, as was the case with squads III. and IV. The parentheses around their averages indicate then that these were not really caffein days, but days on which the caffein squads took the doses indicated at the top of the various columns. The sleep records of the individuals taking caffein may thus be compared with the control records of individuals taking only sugar on the corresponding days. The caffein records are thus trebly checked up—first by a normal week for all subjects, second by control days for all subjects during the following three weeks, and thirdly by a control squad running throughout the month on sugar doses only.

Squad II., consisting of subjects 8, 12 and 13 took caffein on three days and sugar on the following three days, at 10:30, followed in turn by three days of caffein, and so on throughout the experiment, except that there was only one day available for the 6 grain dose. The 1–2 grain records for this squad are the averages of 3 days of 1 grain and 3 days of 2 grain doses. The records for 3–4 grains are the averages of three 4 grain days, while the records for 6 grains are not averages but single records.

Squad III. took caffein and sugar doses on alternate days throughout the experiment, at the 1 o'clock lunch hour, thus giving two days for each of the 1, 2, 3, 4 and 6 grain doses. Squad IV. also alternated caffein with sugar doses throughout the month, with the same distribution of days as in the case of Squad III., the only difference being that the dose was taken in the middle of the afternoon instead of at the lunch hour. Squad III. consisted of subjects 3, 9 and 14, while Squad IV. contained subjects 5, 6, 10, 11 and 16.

Section 1. Experiments with Caffein Alkaloid

Table XCVI. gives the individual averages for quality of sleep during the first four weeks, and also the squad averages, with final averages for the caffein days and a grand average for each column for the three caffein squads.

Table XCVII. brings together the averages for each squad, at the same time giving the mean variation of each average and the

number of cases from which the average is in each case derived. The table also gives the final averages for the three caffein squads, the mean variation of the three squad averages from this final average, and the total number of cases for each kind of dose. The number of cases depends chiefly on the number of individuals making up the squad, and in the case of Squad II. on the fact that 3 grain doses were not administered and that only one 6 grain dose was given.

TABLE XCVI

INFLUENCE OF CAFFEIN ALKALOID ON THE QUALITY OF SLEEP

Squad	Subjects	First Week	Control Days	Caffein Doses 1–2 Gr.	3–4 Gr.	6 Gr.	Caffein Average
I.	1	180	190	(225)	(200)	(150)	(200)
Sugar only	4	200	230	(200)	(200)	(300)	(220)
	7	170	150	(175)	(125)	(100)	(140)
	15	200	230	(225)	(200)	(250)	(220)
	Av.	187	200	(206)	(181)	(200)	(195)
II.	8	220	220	250	266	300	273
Morning	12	200	210	200	200	300	233
Caffein 3 days	13	220	190	200	200	200	200
Sugar 3 days	Av.	213	207	216	220	266	234
III.	3	200	240	275	200	200	225
With lunch	9	220	190	225	200	250	225
Alternately	14	200	230	200	175	200	191
	Av.	213	220	233	192	217	214
IV.	5	230	200	250	250	300	266
Mid P.M.	6	200	190	175	175	250	200
Alternately	10	220	210	175	225	300	233
	11	200	210	250	250	200	233
	16	150	140	150	150	300	200
	Av.	200	190	200	210	270	226
Grand averages of Squads II., III. and IV.		208	206	216	207	251	224

The data presented in the two preceding tables seem to justify the following conclusions concerning the influence of caffein alkaloid, taken in its pure form, on the quality of sleep.

1. Doses smaller than 6 grains do not cause sleep impairment, so far as the squad averages indicate. The average M.V. of these squad averages is 38. Although the figures for Squad II. are larger for all doses of caffein the range is quite within the probable error except for the 6 grain doses where sleeplessness is clearly present

(Table XCVII.). Squad III. shows no sleep impairment whatever, the quality being reported even quite uniformly better on caffein days. Squad IV. similarly shows no deterioration in sleep quality until the 6 grain dose is reached, but the falling off at this point (270) is apparent. The final averages of the caffein squads show the rule clearly. Up to the 4 grain doses the sleep quality remains quite constantly about 209, but at 6 grains there is an abrupt falling off to 251 (Table XCVII.). Since these figures are the averages of 11 individuals, with daily records covering a period of 28 days, they have high reliability.

TABLE XCVII

THE SQUAD AVERAGES FOR CAFFEIN ALKALOID

Squad		First Week	Control Days	1–2 Gr.	3–4 Gr.	6 Gr.
I.	Av.	187	200	(206)	(181)	—
Sugar only	M.V.	47	35	35	30	—
	Cases	24	28	16	16	—
II.—10:30 A.M.	Av.	213	207	216	220	266
Caffein 3 days	M.V.	40	31	27	33	44
Sugar 3 days	Cases	18	30	18	9	3
III.	Av.	213	220	233	192	217
With lunch on	M.V.	31	40	50	15	72
alternate days	Cases	18	30	12	12	6
IV.	Av.	200	190	200	210	270
Mid P.M.	M.V.	33	49	50	36	39
Alternate days	Cases	30	50	20	20	9
Final averages of	Av.	208	206	216	207	251
caffein squads	M.V.	6	10	11	10	23
	Total cases	66	110	50	41	18

2. The influence of the caffein dose depends to a quite appreciable degree on the conditions under which the dose is taken, and especially on the time of day, the contents of the stomach at the time, and the frequency with which the dose is taken. This dependence is indicated by a comparison of the averages for Squads II., III. and IV.

(*a*) The greatest impairment of sleep quality is found in the case of those taking the doses on successive days (Av. 234), and this is true in spite of the fact that the dose was in these cases taken early in the morning, some four hours before Squad IV. and two to two and a half hours before Squad III. Had the dose been taken later in the day the difference would doubtless have been still greater. It is impossible to say, on the basis of the data at hand,

how soon this cumulative effect would be compensated by such processes of adaptation as are well recognized features of drug action.

(*b*) Next to this squad, the greatest sleep impairment is found with the group (Squad IV.) taking the caffein dose late in the afternoon, between meals and on an empty stomach (Av. 226).

(*c*) Quite in line with this fact is the further evidence that when the dose is taken along with food substance, as in the case of Squad III., who took the caffein at the lunch hour, there is absolutely no evidence of sleeplessness. The presence of food substance in the stomach seems not only to retard the action of the drug but to weaken or even completely neutralize its effect, so that the average for Squad III. (214) is actually better than their own average on control days (220), although this difference, since it is quite within the probable error, is not evidence of genuine superior quality. The lowest value for this squad is at the 1–2 grain doses (233) but this large figure is due solely to the one individual (subject 3, Table XCVI.) who reported poor sleep for several nights in succession, on control days (240) as well as on caffein days.

3. These results are confirmed by a more detailed study of the records of the individuals making up the various squads. Three subjects, 3, 13 and 14 are not affected even by the maximal 6 grain dose. Five remain unaffected until the 6 grain dose is reached, these being subjects 6, 9, 10, 12 and 16. Only three individuals of the total eleven show signs of disturbance before the 6 grain dose is reached, these being 5 and 8 (who report sleeplessness as soon as the caffein doses begin and show increasing impairment with larger doses) and 11 who reports 250 for 1–2 and for 3–4 grains but did not seem to be disturbed by the 6 grains.

4. The age of the individual does not seem to influence his susceptibility to the sleep disturbing effects of caffein. The three individuals who report poor sleep after the minimal doses are 24, 27 and 33 years of age, respectively, averaging 28 years. The five who were disturbed only by the 6 grains range from 21 to 33 years, averaging 26. The three who are not affected at all are 22, 27 and 39 years old, averaging 29.3 years. The figures thus show no correlation between age and the ease or degree of sleep disturbance. (See Table XCVIII.).

5. Previous caffein habits do not seem to modify the individual's susceptibility during the experiment. Table XCVIII. indicates for each subject the coffee or tea drinking habits indulged in before the experiments began, and reveals no uniformity. Individuals who were accustomed to the regular use of caffein-containing

beverages are to be found in each group along with those who have never used coffee nor tea, or have used them only occasionally.

6. So far as the present experiment is concerned, no sex differences in susceptibility are disclosed. Four of the subjects in the caffein squads were women. Of these two were affected by minimal doses, one by maximal doses only, and one not at all. Of the men, two were not affected at all, four were affected by the maximal dose only, and one by the minimal.

TABLE XCVIII

Individual Differences in Susceptibility

	Type	Subject	Age	Weight	Caffein Habit
1	Reacted to minimal doses	8	24	144	Used regularly
		5	33	105	Used regularly
		11	27	110	Abstainer
		Average	28	120	
2	Reacted to maximal doses only	12	24	160	Used regularly
		9	21	130	Abstainer
		6	33	125	Used regularly
		10	28	157	Occasionally
		16	24	174	Used regularly
		Average	26	149	
3	No reaction	13	22	175	Used regularly
		3	39	159	Abstainer
		14	27	193	Occasionally
		Average	29.3	176	

7. The only factor which correlates closely with susceptibility is weight. Table XCVIII. shows this correlation clearly. The average weight of those who are affected by the minimal doses is only 120 pounds. The average weight of those who are affected, but only by the maximal dose is 149 pounds, while the average weight of the three individuals who are not affected at all is 176 pounds. That is to say, the greater the body weight of the individual the greater the immunity to the sleep-disturbing influence of caffein. The two lightest subjects, 5 and 11, are in the susceptible group, while the heaviest woman and the two heaviest men are in the immune group. The relation between body weight and the action of drugs in a medicinal way is a well-recognized principle of pharmacology, and it is interesting to find such close confirmation of the principle in these introspective sleep records. It means simply that, when a fixed dose is administered, the heavy or large individual receives relatively a smaller dose, per unit of tissue, and the influence of the drug is

correspondingly reduced. Age and sex differences in the susceptibility to drugs are probably in many cases not true age or sex differences but differences based on variations in weight or size. A given amount of caffein, for instance, should be expected to produce greater sleep disturbances in a child than in an adult, not so much because of the child's immaturity as because of the fact that he receives a larger dose per unit of tissue.

Table XCVIII. presents the classification of the eleven subjects according to their susceptibility, giving at the same time their age, weight and caffein habit.

TABLE XCIX

Effect of Caffein on Amount of Sleep

		1st Wk.	Control	1–2 Gr.	3–4 Gr.	6 Gr.	Caf. Av.
Squad I.	Sugar only	7.45	7.40	(7.50)	(7.75)	(7.67)	(7.55)
II.	Three-day periods	7.13	7.63	7.46	7.57	7.07	7.37
III.	With lunch	7.43	7.47	7.57	7.33	6.40	6.90
IV.	Middle P.M.	7.20	7.46	7.38	7.00	6.30	6.88
Average of Caffein Squads II., III. and IV.		7.25	7.52	7.47	7.30	6.59	7.05

Table XCIX. gives the squad averages of the approximate number of hours' sleep, as well as the final average for the three caffein squads. The figures indicate hours and decimal parts of an hour. The indications here thoroughly bear out the conclusions based on the judgments of sleep quality. The control squad (I.) shows little variation as the experiment proceeds, the average amount of sleep being uniformly about 7.5 hours. Squad II., taking caffein in the morning, proceeds in much the same way until the 6 grain dose is given, when the time drops to about 7 hours, a loss of half an hour sleep. The amount of sleep for Squad III., taking dose with lunch does not diminish appreciably until the 6 grain dose is given, when the earlier average of 7.4 hours abruptly drops to 6.4 hours, a loss of one hour. The figures for Squad IV., taking dose in the afternoon, between meals, drops off .5 hour at the 3–4 grain doses, and still another .5 hour at the 6 grain dose. Comparing the caffein average with the control average, Squads III. and IV. lose on the whole for 1–6 grain doses, .6 of an hour, while Squad II. averages, for the whole range of doses, a loss of only .3 of an hour. The grand average of the caffein squads shows a slight tendency to fall off for the smaller doses, with an abrupt loss of .7 of an hour at the 6 grain dose, while the grand average for the total range of doses (1–6 grains) shows a loss of half an hour, the figure falling from 7.5 hours on control days to 7.0 hours. The individual records show nothing new, and are consequently not given.

SECTION 2. INFLUENCE OF SYRUPS, WITH AND WITHOUT CAFFEIN, ON THE QUALITY OF SLEEP

This experiment covered one week. On two days no dose at all was given. On two days soda fountain syrup, containing no caffein, was served with carbonated water. On the remaining three days, which were, however, scattered throughout the week, varying amounts of the syrup were given, served with the carbonated water as before, but containing caffein alkaloid (1.2 grains per glass). On one day 1 glass was taken, on another 3 glasses and on the third day 5 glasses. In all cases the drink was taken in the middle of the afternoon. When the large amounts were taken the drinks were distributed over a period of 2 or 2.5 hours. When the small amounts were taken the 3:00 o'clock hour was chosen for the drink. In this experiment 12 subjects were used, all of whom had taken part in the previous experiment. No squad division was made, the days on which no dose was taken (called "blank days" in the table), served as control days for the whole group.

TABLE C

INFLUENCE OF SYRUPS, WITH AND WITHOUT CAFFEIN

Character of the Dose	Sleep Quality Average	Sleep Quality M.V.	Hours Sleep Average
Blank days	179	54	7.48
Plain syrup, with carbonated water	208	23	7.69
Syrup, with caffein,			
1 glass (1.2 gr.)	180	30	7.42
3 glasses (3.6 gr.)	209	49	7.16
5 glasses (6.0 gr.)	250	60	6.75
Syrup, with caffein, average	213	46	7.11

Representing as before, the *normal* quality of sleep by the value 200, letting 100 represent sleep introspectively judged as *better than usual,* and 300 sleep judged as *worse than usual,* the following table (C.) results from the week's experiment on the 12 subjects. The figure for "blank days" is thus an average of 12 records on each of 2 days an average of 24 cases. The figure for "syrup days" is an average, in the same way, of 24 cases, while each of the "caffein" figures is an average of 12 records.

The results of this experiment completely confirm the conclusions presented in the first section. The "blank days" have a good average quality (179), but no better than that for the day of the 1 glass of syrup drink, containing 1.2 grains of caffein. The 3.6 grain dose of caffein results in a poorer quality of sleep than that on

the "blank days," but no poorer than that reported on the days on which the same amount of plain syrup was taken (3.6 gr. caffein—209, plain syrup—208). Moreover both these figures are approximately ordinary sleep (200). But on the days on which the drink contained 6 grains of caffein there is clear evidence of sleep impairment (250). This is just the point at which the doses of pure caffein alkaloid produced marked signs of sleep disturbance. The average for the three caffein days (1–6 gr.) is poorer than that for the plain syrup days (caffein 213, syrup 208), while the average for "blank days," is better than either of these two (179).

The reports of approximate amount of sleep point in the same direction. The "blank days," the plain syrup days and the 1.2 gr. caffein day all yield the well established normal of about 7.5 hours of sleep. The average after the 3.6 gr. caffein dose appears to be only slightly less, being 7.16, a loss of about .25 hour. But after the 6 grain dose there is an abrupt falling off, the average being only 6.75 hours, a loss of .75 hour as compared with the normal amount. Not only in their relative amounts, but in absolute magnitude as well, the figures for sleep quality and for amount of sleep, in this second section, correspond closely to the figures yielded by the experiments reported in the preceding section on the effects of pure caffein doses.

It should be remarked that in no case did the subjects know the nature of the dose which they were taking at the time. The only indications they had throughout the experiment were based on the after effects of the drug. In Section 1, each subject received a capsule daily, this capsule sometimes containing caffein in amount known only to the director of the experiment, and at other times only sugar of milk. The control capsules were varied in size, as were the caffein capsules of necessity. In Section 2, the subjects knew only that they were taking soda-fountain drinks. There was said to be a slight variation in the taste from time to time, but this was supposed, by the subjects, to be due to the degree to which the syrup had been mixed with the carbonated water, to its temperature, etc. The disturbing factors of suggestion, interest, excitement and unequal introspective attention were thus effectually avoided.

Summary

By way of summary we may say: Small doses of caffein alkaloid (1–4 grains), taken either in the pure form or accompanied by small amounts of syrup, do not produce appreciable sleep disturbance except in a few individual cases. Doses larger than these

(6 grains, in the present experiment) induce marked sleep impairment with most subjects, though even here a few individuals show complete resistance to its effects. The effects are greatest when the dose is taken on an empty stomach or without food substance, and when it is taken on successive days. The effect of the drug does not seem to depend on the age, sex, or previous caffein habits of the individual, but varies inversely with increase in body weight. These conclusions hold both for the quality and for the amount of sleep.

CHAPTER XIV

The Influence of Caffein on General Health

The individual health record which each subject kept throughout the experiment has already been described in the section on "Supplementary Information" in Chapter II. Since the investigation was concerned chiefly with proficiency in the mental and motor tests, the chief value of the daily health book lay in its service as a check on the records made by the subject and as a guide to the director in the administration of the doses. It was for these two purposes that the health introspections were originally recorded. No definite attempt was made to study the physiological action of caffein on the human system, although the conditions of the experiment afforded excellent opportunity for such observations to be made on at least several of the special organs. The introspections recorded in the health book were more or less general in character whereas only special and detailed examination would have served to determine any local physiological effects that might have resulted from the caffein administration.

Nevertheless examination of the daily reports suggests that they have in themselves a certain positive value, aside from their bearing on the test records. It is not often that careful introspections of 16 individuals over a period of 40 days of carefully controlled drug administration can be presented. Moreover, since the general feeling of well-being will be reflected in the individual's efficiency in the performance of mental and motor tests, we have in these daily records an indirect measure of just those factors which it was the chief purpose of the investigation to examine. Comparison of these reports with the actual changes in efficiency as measured by the tests shows a close correlation between the two sets of results. For these reasons it seems worth while to publish the daily introspections in full. The following pages of this chapter contain the daily records of all the 16 subjects from February 10 to March 2, the period of three weeks during which the caffein doses were administered in Experiment *A*. The records have been copied directly from the individual books, the only changes consisting of occasional abbreviations and grammatical corrections. The records of each of the subjects who received caffein are followed by a brief statement

of general results in the case of the subject in question. At the close of the chapter is given a general statement of the results for each squad. These results are stated only tentatively, completer interpretation being left for such readers as have fuller medical knowledge than the writer.

At the close of the experiments each subject was asked whether or not he had read, during the investigation, any general or special treatise concerning the supposed effects of caffein, and whether or not he felt that his introspections had been in any way influenced by suggestions received in this way, or by his general knowledge or expectation. No subject had ever read any account of the action of caffein other than the incidental references to it found in the textbooks of physiology used in the elementary schools.

The following four subjects comprised the control squad and ran through the whole period on sugar doses only, given at 1:00 P.M.

Subject 1, M., 39, , Regular User

Feb. 10. Felt and slept as well as usual since the experiments began.

11. Felt duller than usual to-day. Slept normally, 6.25 hrs.

12. Felt normal to-day aside from a slight headache in the morning. Much better than yesterday. Slept about 6 hrs., but woke up tired a full hour before necessary. Sleep fully normal while it lasted.

13. Felt duller than yesterday but not so dull as Saturday. Slightly constipated. Slept soundly about 6 hrs., being awakened by noise next door. Slept better than usual since the experiments began.

14. Felt normal to-day. Slept soundly for about 6 hrs.

15. Felt fine this morning but dull this afternoon. Slight inclination to headache in afternoon but this gave way to a heavy feeling in the head. Slept not so well as usual, about 5.75 hrs.

16. Felt depressed during the morning but better in the afternoon. Headache and dull feeling gone in afternoon. Slept as soundly as usual for 6 hrs.

17. Normal during morning and early part of the afternoon, but about the middle of the afternoon I seemed to have a slight fever and from that time on did not feel so bright as usual. Slept normally for 6 hrs.

18. Felt normal to-day except in the matter of nervousness which may have been due to the completion of some work not connected with the experiments. Have certainly been more nervous than before the experiments began. Slept as well as usual, 6 hrs.

19. I seemed to be normal up to 11:00 A.M., when a dizzy spell lasting about 20 minutes began, which was succeeded by a dull but not severe headache. In the P.M. between 2:00 and 4:00 the headache seemed slightly more troublesome. Was awakened in the middle of the night by no apparent cause but went to sleep again probably in half an hour. 6 hrs.

20. Felt normal this morning but got drowsy about 3:00 P.M. and have felt duller than usual since. Slept as well as usual about 6 hrs.

21. Felt normal to-day, but slightly nervous. Slept as well as usual.
22. Normal during morning. In afternoon a slight headache came on together with a slight fever. Felt dull since about 2:30. Slept worse than usual, waking early. About 5 hrs.
23. Felt normal to-day with exception of slight tendency to nervousness and weakness. This condition has been uniform throughout the day. Slept as soundly as usual, about 5.75 hrs.
24. Aside from a tired feeling around the eyes I have felt as well as usual to-day. Between 11:00 and 12:00 I seemed to become slightly flushed but was probably not feverish. Felt better in the afternoon. Slept soundly as usual for 6 hrs.
25. Felt slightly better than usual to-day. Slept about 6 hrs., but did not wake so refreshed as yesterday.
26. A little tired early in the morning but soon recovered my usual form. Slight tendency toward headache toward noon and after that felt duller than usual for about three hours. Slept as usual, for approximately 6.25 hrs.
27. Felt dull this morning and had shooting pains in the back of my head especially after 11:00 A.M. Felt much brighter in the afternoon, and as well as usual. Slept better than usual, for 6.25 hrs.
28. Felt better than usual all day. Not one unfavorable symptom. Slept as well as usual for about 6 hrs.

Subject 4, M., 19, 124, Moderate User

Feb. 10. Felt as well as usual duiing the day. During the evening I felt more wide awake than usual. Sleep ordinary, 8.5 hrs.
11. As usual during day. Sleep as well as usual, 7.5 hrs.
12. As usual during day. Sleep as well as usual, 8 hrs.
13. As usual during day. Sleep as well as usual, 8 hrs.
14. Felt as well as usual. Did not sleep well toward morning but woke up several times. 8 hrs.
15. As usual during day. Sleep as usual. 8 hrs.
16. As usual during day. Sleep as usual. 8 hrs.
17. As usual during day. Sleep as usual. 7.5 hrs.
18. As usual during day. Sleep as usual. 7.5 hrs.
19. As usual during day. Sleep as usual. 8 hrs.
20. Felt as usual. Sleep poorer than usual. 8 hrs.
21. Felt as usual. Sleep as good as usual. 8 hrs.
22. Felt as usual. Sleep as good as usual. 8 hrs.
23. Felt as usual. Sleep as good as usual. 8 hrs.
24. Did not feel as well in the P.M., being quite tired. Did not sleep quite as well as usual. 7.5 hrs.
25. Slight headache all day. Slept as well as usual. 7.5 hrs.
26. Slight headache all day. Slept as well as usual. 7.5 hrs.
27. Felt as well as usual during day. Did not sleep quite as well as usual. 8.5 hrs.
28. Felt as usual during the morning. Slept as well as usual, 8.5 hrs.

Subject 7, M., 19, 153, Moderate User

Feb. 10. Feeling as usual. Slept a little better than usual, 7.5 hrs.

11. Quite drowsy in forenoon and early afternoon. Later felt as usual. Slept brokenly, but longer than usual, 9.5 hrs.

12. A little drowsy about noon. Later as usual. Slept as usual, 7.5 hrs.

13. More tired than usual to-day. Slept unbrokenly for 6.5 hrs.

14. More tired than usual. Slight headache. Slept 7 hrs., more soundly than usual.

15. Am feeling a little tired, more than usual. Was quite sleepy in the afternoon. Slept about 8 hrs., which is a little more than usual. Slept evenly and a little better than usual.

16. Felt better than usual during the day. Slept better than usual, for 9 hrs., unbrokenly.

17. Felt better than usual during day. Have slight headache now. Slept about as usual for about 9 hrs.

18. As usual during the day, except for slight headache at the beginning of the forenoon, which steadily diminished. Slept about as usual, for 7.5 hrs.

19. Felt very sleepy during the day. Have a slight headache now (6:30). Slept 9 hours, about as usual.

20. A little drowsy in afternoon and headache in evening. Slept about as usual for 7 hrs.

21. Felt better than usual during day, but have slight headache this evening. Slept a little better than usual for 9 hrs.

22. Felt a little drowsy in afternoon, otherwise a little better than usual. Slept about as usual for 10 hrs.

23. Felt about as usual during day except for slight headache toward evening. Slept 8 hrs. about as well as usual.

24. Better than usual during day with slight headache toward evening. Slept better than usual for 7.5 hrs.

25. As usual during morning with headache in afternoon. Slept about as usual for 8 hrs.

26. Felt very badly all during the day, which is not as usual. Have an intense headache now. Slept as usual for about 9 hrs.

27. Felt as usual to-day. Slept a little better than usual for about 8 hrs.

28. Felt a little better than usual to-day. A little blue toward evening, with a slight headache. Slept about as usual for about 7 hrs.

Subject 15, F., 34, 108, Occasional User

Feb. 10. Felt and slept about as usual. 8.25 hrs.

11. Felt better and more rested than I have since the tests began. Slept 8 hrs. as well as usual.

12. Felt and slept about as well as usual. 8 hrs.

13. As well as usual except at 10:00 A.M., when I was nervous on account of the baby crying so for me. Slept as usual, 8 hrs.

14. Perhaps a little more tired than yesterday. Didn't sleep so well as usual. 7 hrs.

15. Slept and felt as well as usual.

16. Began as usual but got tired and nervous in the afternoon. Slept as usual, but now have unpleasant dreams oftener. 8.5 hrs.

17. As well as usual and much less tired than yesterday. Did not sleep as well as usual on account of the wind. 8 hrs.
18. Felt as usual. Not tired at all as I am some evenings after the day's work. Slept as usual. 7.5 hrs.
19. Felt and slept as usual to-day. 8 hrs.
20. Felt as well as usual until 5:30, then I had a slight headache. Did not sleep quite so well as usual. 7.5 hrs.
21. (—)* As well as usual. My eyes have been troubling me a great deal for the past week. Slept as well as usual. 8 hrs.
22. As well as usual excepting a slight headache in afternoon. Sleep as good as usual. 8 hrs.
23. Felt dull all day, and had headache at evening test. Slept as well as usual after I got to sleep, but was kept awake later with the baby. Also had headache. 7 hrs.
24. (—) Felt as well as usual. Did not sleep as well as usual and had bad dreams. 7.5 hrs.
25. As well as usual, but tired at the last test. Sleep as usual. 9 hrs.
26. Felt as usual all day. Slept as well as usual. 8.5 hrs.

Subject 3, F., 39, 159, Abstainer, Squad III

Feb. 10, 1 gr. sugar, 1:00 P.M. with lunch. (Morning) Feel as usual to-day. (Evening) Feel as usual, not much tired. Went to bed at 10:30, had slight sore throat and slept poorly. Woke at 6:45.

Feb. 11, 1 gr. caffein, as above. (Morning) Feel all right. (Evening) Felt as usual all day. Slept better than last night, went to bed at 11:00 and woke at 6:30.

Feb. 12, 1 gr. sugar, as above. (Morning) Feel as usual this morning. (Evening) More tired than usual. No other difference. Went to sleep at 9:15, woke at 6:30. Slept poorly the latter part of the night. Sleep not so good as usual.

Feb. 13, 1 gr. caffein, as above. A little tired this morning. (Evening) Less tired than this morning. Felt as usual to-day. Went to bed at 10:30, woke at 6:45. Slept better than night before but not so well as usual.

Feb. 14, 2 gr. sugar. (Morning) feel as usual. (Evening) Less tired than yesterday and day before. Otherwise as usual. Bed at 10:30, woke at 6:30, did not sleep as well as usual the latter part of the night.

Feb. 15, 2 gr. caffein. (Morning) Feel as usual. (Evening) Felt as usual all day, bed at 10:30, woke at 6:30, did not sleep as well as usual.

Feb. 16, 2 gr. sugar. (Morning) As usual this morning. (Evening) Felt as usual all day. Slept almost as well as usual (8 hrs.), woke with a slight headache.

Feb. 17, 2 gr. caffein. (Morning) (—) As usual this morning. (Evening) No headache since noon. Slept as well as usual, 7.5 hrs.

Feb. 18, 3 gr. sugar. (Morning) As usual. (Evening) Unusually tired this afternoon and sleepy. Feel better in evening, after short rest. Slept from 11:00 to 6:30, as well as usual.

Feb. 19, 3 gr. caffein. (Morning) As usual. (Evening) Less tired than usual. Slept from 10:00 to 6:30, as well as usual.

Feb. 20, 3 gr. sugar. (Morning) Feel all right. (Evening) Tired and nervous after lunch, feel all right this evening, and as usual. Slept as well as usual, from 11:20 to 6:30.

* These marks (—) indicate beginning and end of menstrual period.

Feb. 21, 3 gr. caffein. Felt a trembling sensation all day, more noticeable this afternoon. Otherwise as usual. Slept 7.5 hrs. as well as usual.

Feb. 22, 4 gr. sugar. (Morning) As usual. (Evening) As usual all day. (—) Slept 8 hrs. very well, as usual.

Feb. 23, 4 gr. caffein. (Morning) As usual. (Evening) More nervous than usual after lunch. Otherwise as usual. Slept 8 hrs., well as usual.

Feb. 24, 4 gr. sugar. (Morning) Normal. (Evening) Have felt as usual to-day, have not been nervous, as was the case yesterday. Slept 8 hrs. as well as usual.

Feb. 25, 4 gr. caffein. (Morning) Feel all right this morning. (Evening) As usual to-day, slept from 11:30 to 6:45.

Feb. 26, 6 gr. sugar. (Morning) Feel as usual. (Evening) Drowsy in the afternoon, slept as usual, but over 9 hrs.

Feb. 27, 6 gr. caffein. (Morning) As usual. (Evening) Slightly dizzy at 1:45. Been nervous during the afternoon. Baby kept me awake until 12. Couldn't go to sleep before 1:00. Slept poorly until 6:30.

Feb. 28, 6gr. sugar. (Morning) Feel all right. (Evening) As usual all day. Slept almost as well as usual, 6.5 hrs.

March 1, 6 gr. caffein. (Morning) Feel as usual, all forenoon. (Evening) Have been nervous and unsteady this afternoon, slept from about 11:00 to 6:40, and about as well as usual.

March 2, 6 gr. sugar. Felt as usual to-day, except that I seemed to have less energy after about 12:00 o'clock.

General Result.—Nervous feeling from doses of 3, 4, and 6 gr. of caffein. Suggestion of dizziness and sleeplessness following the first 6 gr. dose.

Subject 9, M., 21, 130, Abstainer, Squad III

Feb. 10, 1 gr. sugar, 1:00 P.M., with lunch. (Morning) As well as usual. (Evening) As usual through day. Lost three hours sleep due to screaming of woman in same apartment house.

Feb. 11, 1 gr. caffein, as above. As usual throughout the day. Slept as usual, 7 hrs.

Feb. 12, 1 gr. sugar. As usual all day. Same for sleep.

Feb. 13, 1 gr. caffein. As usual all day. Same for sleep.

Feb. 14, 2 gr. sugar. As usual all day. Same for sleep.

Feb. 15, 2 gr. caffein. As usual all day. Same for sleep.

Feb. 16, 2 gr. sugar. As well as usual, drowsy all day, especially in the forenoon. Slept as usual.

Feb. 17, 2 gr. caffein. Felt as usual. Sleep poorer than usual, slept only 6.5 hrs.

Feb. 18, 3 gr. sugar. Sleep and condition same us usual.

Feb. 19, 3 gr. caffein. Everything as usual.

Feb. 20, 3 gr. sugar. Felt as usual. Slept better than usual, 7.5 hrs.

Feb. 21, 3 gr. caffein. (Morning) As well as usual. (Evening) A little nervous in the afternoon. Slept 7.5 hrs. and as well as usual.

Feb. 22, 4 gr. sugar. Everything as usual.

Feb. 23, 4 gr. caffein. (Morning) Well as usual. (Evening) Pain in stomach in afternoon, also very nervous. Slept as usual.

Feb. 24, 4 gr. sugar. Everything as usual.

Feb. 25, 4 gr. caffein. Everything as usual.

Feb. 26, 6 gr. sugar. Everything as usual.

Feb. 27, 6 gr. caffein. (Morning) as usual. (Evening) Nervous in the afternoon. Slept as usual.

Feb. 28, 6 gr. sugar. Everything as usual. Slept two hours in the afternoon. Evening sleep as usual.

March 1, 6 gr. caffein. (Morning) As usual. (Evening) Nervous in the afternoon. Did not sleep well (lost three hours' sleep on account of a robber in the house).

March 2, sugar. As well as usual. Sleepy in afternoon.

General Result.—Nervousness for doses of 4 and 6 gr. No indications of sleep disturbance as result of caffein. Stomach pains mentioned once, after 4 gr. caffein dose.

Subject 14, M., 27, 193, Occasional User

Feb. 10, 1 gr. sugar, 1:00 P.M., with lunch. Felt almost ordinary to-day (this subject had been having a bad cold). Cold is disappearing and nose bleed has not bothered me any. Slept as usual, 8 hrs.

Feb. 11, 1 gr. caffein, as above. No difficulty this morning. (Evening) Have felt better to-day than for a week. Slept better than usual for 8 good hours.

Feb. 12, 1 gr. sugar. Slight headache about 9:30 A.M. (Evening) Feel better than usual, and have all day. Slept as usual for 8 hrs.

Feb. 13, 1 gr. caffein. (Morning) Feeling better than usual, or at least better than first week of experiment. (Evening) As usual all day. Slept as usual for 8 hrs.

Feb. 14, 2 gr. sugar. (Morning) Left ear slightly inflamed and sore. (Evening) With exception of my ear, which is feeling better this evening, I have felt normal all day. Have a slight heartburn late this evening, but not sufficient to make me feel worse than usual. Slept better than normal for 9 hrs.

Feb. 15, 2 gr. caffein. (Morning) Ear about as yesterday. Dropping sweet oil in it. (P.M.) Feel just a little tired this evening, especially in the arms, due, I think, to the tapping test, which seems to tire me. Would say not quite normal. Slept as usual, for 7 hrs.

Feb. 16, 2 gr. sugar. (Morning) Tonsil still swollen slightly and left ear still sore (prescriptions given by medical assistant for ear, throat and general system). Felt somewhat dull all morning. (Evening) Found that I was able to do some fair studying from 1:30 to 3:00 P.M. and during the laboratory periods. I do not feel as full of energy as is normal for me. Slept worse than usual, for 8 hrs. Woke up several times.

Feb. 17, 2 gr. caffein. Felt bad all day, but not particularly dull. Ear does not bother me any more. Have a sort of heartburn. Since I have not been taking regular exercise I started in with a run at the gym before the 5:30 test and think the heating up entered into my evening records. Slept not quite normal for 8 hrs.

Feb. 18, 3 gr. sugar. Have not felt normal to-day, due to my extremely sore throat. Burning sensation away down in my throat all the time. Have a feeling of fatigue. Slept well for about 7.5 hrs., better than on two former nights, so almost normal.

Feb. 19, 3 gr. caffein. (Morning) Felt much better than yesterday. (Evening) Felt as usual to-day. Ear and throat better. Do not feel fatigued as last night. Slept normal for 7.5 hrs.

Feb. 20, 3 gr. sugar. Felt as usual for the day. Ear and throat still sore. Not particularly bright so far as intelligence goes. Seemed hard for me to follow cases in law class at 2:00 P.M. Slept as usual for 7 hrs.

Feb. 21, 3 gr. caffein. (Morning) My eyes are tired and rather strained. (Evening) Felt normal to-day, but eyes continue tired. Slept better than usual, and was also better able to study than usual in the late evening.

Feb. 22, 4 gr. sugar. Normal all day. Eyes not quite so tired. Slept better than usual, for about 7 hrs.

Feb. 23, 4 gr. caffein. (Morning) Felt better than usual all morning, and up until about 2:30 P.M., when I began to feel rather shaky, nervous. Through the test I felt queer and wanted to get outside and wear off my nervousness. No headache, but the muscles of my arm seem shaky. Don 't seem to have the usual control over them. Feel a kind of burning sensation from my stomach and on up to my throat. Slept as usual for 7.5 hrs. Was awakened once along towards morning, but had no trouble in going back to sleep again.

Feb. 24, 4 gr. sugar. Felt quite normal all day. Have not felt nervous as yesterday afternoon. Slept for 8 hrs., about as usual.

Feb. 25, 4 gr. caffein. I felt, I should say, better than usual until this afternoon, about 3:00 P.M. However I did not feel so nervous as day before yesterday afternoon (Feb. 23), until during the last test (5:30–6:30 P.M.). My lower arms feel sort of wobbly as before. The average for the day would be about normal or a little below if anything. Slept as usual for 7 hrs.

Feb. 26, 6 gr. sugar. Felt normal all day. No nervousness in forearms as on some other afternoons. Slept as usual, 7 hrs.

Feb. 27, 6 gr. caffein. Felt normal all day until about 2:00 P.M. I was trying to read some cases in Constitutional Law and suddenly found that I was making little headway. Noticed a headache come on me which gradually got worse until about 3:30 P.M. Head ached in front above my eyes. My ears burned and are still burning. This is the first time I have had a headache during the experiment. I don 't remember having had a headache for several months. After 4:00 P.M. I got out in the air with Ames and walking down to 125th St. we bowled a couple of games. During the first game I was suddenly conscious that my head was not aching. I now feel much better except that my forearms feel nervous and my ears are still burning. Have also, since 5:30 been feeling slightly sick at the stomach. Slept as usual for a little over 8 hrs.

Feb. 28, 6 gr. sugar. Nothing unusual or exciting to-day, except the fact that I felt awfully drowsy while studying here in the laboratory from 2 to 3 P.M. On the whole have felt quite normal. Slept fine for a good 9 hrs. I do not have any difficulty in going to sleep, nor has my sleep been restless. Have always heard that coffee kept one awake and caused them to awaken during the night. I have had no trouble in going to sleep, and my sleep has, almost without exception, been sound.

March 1, 6 gr. caffein. Felt normal up to about 3:30 P.M., when I began to feel somewhat nervous, but not nearly so bad as on former days, and particularly day before yesterday. Went to the gym 4 to 5 P.M., and while feeling very near normal now (5:30) I am weak and the muscles of my lower arms feel peculiar and unsteady. Slept a little less than 7 hrs., and not quite as well as usual.

March 2, sugar. I have felt normal all day. Have not felt any marked nervousness as on some previous days.

General Result.—No indications of impaired sleep. Nervousness after doses of 4 and 6 grains of caffein. Suggestions of heartburn and stomach pains after large doses. Headache after 6 gr.

Subject 5, F., 105, Regular User, Squad IV

Feb. 10, 1 gr. sugar, middle of afternoon. My head has felt dull all day, but was more clear just at noon than at any other time. (Evening) Feel more tired than usual. Slept 8 hrs., about as well as usual.

Feb. 11, 1 gr. caffein. About as usual to-day. Sleepy and tired in evening. Slept 7 hrs., and about as usual.

Feb. 12, 1 gr. sugar, as above. About as usual all day. (—) Slept quite well, usual time.

Feb. 13, 1 gr. caffein, as above. Felt about as usual. Very stupid just after lunch. Did not sleep as well as usual owing to commotion in the neighborhood. About 6.5 hrs.

Feb. 14, 2 gr. sugar. To-day I have had to force myself more than usual to try to keep up to my ordinary work. Am so hungry just now that my head aches, and I feel all gone. (Evening) Felt pretty well, but could not go to sleep till after midnight. Then slept as well as usual.

Feb. 15, 2 gr. caffein. (Morning) Felt about as usual. (Evening) A little light-headed and dizzy this afternoon. During the last few tests I was very faint and had a queer sick feeling at times. My mouth felt quite as if the saliva would start faster than I could swallow it and my limbs had a numb feeling after I reached home. Went to bed directly after dinner and rested as well as usual, sleeping about 9.5 hrs.

Feb. 16, 2 gr. sugar. (Morning) A bit weak and light-headed. During the early part of the day I felt very tired. These feelings gradually wore off and this evening I feel pretty well except a little more tired than usual. Slept about as usual, for 8 hrs.

Feb. 17, 2 gr. caffein. All day have felt very dull. During the middle of the day my head ached quite a little but toward evening it cleared up some, although it still aches. During the early evening this headache grew much worse, the tense feeling at the back of the head made it feel as if something would burst. By taking a very hot bath I managed to relieve it somewhat, but slept much worse than usual, for about 6 hrs.

Feb. 18, 3 gr. sugar. All day have had a very tense feeling at back of head. Just before lunch my head felt as if something in it would snap. After resting this feeling was relieved but is not all gone. My throat is somewhat sore to-night. Slept as usual, 7 hrs.

Feb. 19, 3 gr. caffein. Head has felt lame all day, not enough to call a headache except just after lunch, when it felt very drawn and tight. Much better after resting. Slept as usual till 5 A.M., when aroused by noises in the court. 6.75 hrs.

Feb. 20, 3 gr. sugar. Felt quite normal to-day, very little of the tight feeling in my head. Slept as usual, about 8 hrs.

Feb. 21, 3 gr. caffein. Felt better than usual to-day. Slept as well as usual, 8 hrs.

Feb. 22, 4 gr. sugar. Felt about as usual to-day, except just after lunch, when my head had quite a little of that drawn feeling at the back. Slept not quite as well as usual, for about 7 hrs.

Feb. 23, 4 gr. caffein. (Morning) About as usual. (Evening) This afternoon during the last two tests I have felt excited and very trembly. My arms feel very weak and it is hard to control the muscles. In walking back from home for the 3:00 o'clock test I felt light-headed, but otherwise perfectly well. Did not sleep quite as well as usual, could not go to sleep for some time after going to bed. About 6 hrs.

Feb. 24, 4 gr. sugar. A cold in my head has made it very hard to concentrate to-day. Congested feeling in the head, and since the latter part of the afternoon my head has ached a little, as well as every bone in my body. Am very tired this evening. Slept very soundly, and better than usual, about 8.5 hrs.

Feb. 25, 4 gr. caffein. (Morning) about as usual. (Evening) Just after lunch was very sleepy and dull. Very nervous during the last tests on account of a visitor to the laboratory. Slept much worse than usual, could not close my eyes for some time and had a drawn feeling at the back of my head again. Very nervous and did not go to sleep till after 1:00 o'clock, then slept for about 4.5 hrs.

Feb. 26, 6 gr. sugar. All day have been exceedingly tired, just after lunch felt almost exhausted and very sleepy. Drawn feeling at back of head. Slept about as usual, but awakened with a very bad headache.

Feb. 27, 6 gr. caffein. (Morning) Head ached very hard all morning, the pain being hardest just after lunch. (Evening) All the afternoon, while my head has been better, I have felt very weak and light-headed and even sort of faint. Arms and limbs have had a numb sort of feeling, and have been very cold. After resting at 1:30, on getting up found the veins in arms and hand very swollen. Felt very weak all during the evening, and for awhile my ears felt as if something in them would burst. Retired early but could not go to sleep for a long time, not till midnight. Slept very lightly about 5.5 hrs.

Feb. 28, 6 gr. sugar. Have been about as usual to-day, was unable to take my usual afternoon's sleep, being perfectly wide awake. At night was wakeful for a short time, then slept about as usual, 7 hrs.

March 1, 6 gr. caffein. (Morning) Some tired, but felt all right otherwise. After lunch, while at home, began to feel badly, my head felt very light and my arms and limbs numb. Brain unusually active, so much so that it gave me a wild kind of feeling and I did not like to stay alone. (Evening) After coming back to the laboratory the light-headed feeling grew worse. I scarcely knew what I was doing and my limbs were very cold. It was extremely hard for me to breathe, and the beating of my heart seemed to choke me. Was very restless at night, did not go to sleep till after midnight, and then awakened a number of times for just a few minutes. Dreamed all night. Slept worse than usual, for about 5.5 hrs.

March 2, sugar. Have had some sore throat all day, and have been tired, a little more so than usual.

General Result.—The symptoms reported by this subject were all present in her accounts of the first week during which sugar doses only were given. That she was very nervous and quite unfitted for the strenuous life required during the experiment is obvious. Several spells characterized by fainting and chill and numbness, occurring during the course of the experiment, required the aid

of the medical adviser. These attacks occurred on control days as well as on caffein days. The drawn feeling in the head, the tenseness and difficulty in sleeping were present before the caffein doses began. This condition was no doubt aggravated by the severe strain of undergoing the tests, but it is also obvious that all the symptoms were intensified by the larger caffein doses. The drawn feeling in the head was more pronounced and the numbness and coldness and lightheadedness are more frequently mentioned and seem to have been more annoying. Impairment of sleep on caffein days is also apparent for doses larger than 2 gr.

Subject 6, F., 33, 125, Regular User, Squad IV

Feb. 10, 1 gr. sugar, middle of afternoon. Felt unseated all morning, but as usual again at 6:30. Eyes tired. Slept well as usual. 7 hrs.

Feb. 11, 1 gr. caffein, as above. As usual. Did not feel at all tired until the last hour. Slept as usual, for 7 hrs.

Feb. 12, 1 gr. sugar. As usual. Slept well as usual from 11 to 2. Very brokenly from then to 6:30. Think it was due to nervousness from being alone.

Feb. 13, 1 gr. caffein. Felt and slept as well as usual. 7 hrs.

Feb. 14, 2 gr. sugar. Felt and slept as well as usual. 6 hrs.

Feb. 15, 2 gr. caffein. As usual except for aching and weakness in arm. Slept as well as usual, but for only 5 hrs.

Feb. 16, 2 gr. sugar. As usual except very sleepy, probably due to loss of sleep last night. Slept as usual for 6.5 hrs.

Feb. 17, 2 gr. caffein. Unusually sleepy about noon, probably due to the weather. Slept better than usual, for 6 hrs.

Feb. 18, 3 gr. sugar. Everything as usual. Arms feeling better. Slept as usual for 7 hrs.

Feb. 19, 3 gr. caffein. No change, except unusually shaky in my arms. Slept as usual, for about 6 hrs.

Feb. 20, 3 gr. sugar. Very sleepy about noon, but feeling as usual at 6:30. Arms steadier than yesterday. Slept as usual, about 6 hrs.

Feb. 21, 3 gr. caffein. Severe headache during the first hours of the day. Felt as though I had a tight band round my head. As usual at 6:30. Slept better than usual, for 7 hrs.

Feb. 22, 4 gr. sugar. As usual. Slept better than usual for 7.5 hrs.

Feb. 23, 4 gr. caffein. As usual until the last period. So weak and shaky during that hour that I could scarcely control any muscle in my body. Also a choking sensation in my throat. Head hot and hands and feet cold. Slept more soundly than usual, for 7.5 hrs.

Feb. 24, 4 gr. sugar. Felt very miserable all day without any decided pain. Aching in back and limbs, a little headache through my temples, a dimness in my vision at times, and my head hot and hands and feet cold were the chief characteristics. My nerves seemed to be on a strain all the time. Slept as usual, for about 8 hrs.

Feb. 25, 4 gr. caffein. Felt some better than yesterday. Slight headache during the middle of the day and weakness in arms and back all day. Very nervous during the last period. Think it was due to visitors watching the tests.

Slept very lightly and brokenly, for about 6 hrs. Extremely nervous all night, with pain in back of head. Retired at 11:00 but could not go to sleep for more than an hour, which is unusual.

Feb. 26, 6 gr. sugar. A little nervous all day and some pain in the back of my head. Slept better than the night before, but not so soundly as usual. About 7 hrs.

Feb. 27, 6 gr. caffein. Tense feeling in back of head all morning. Felt better during the middle of the day, but extremely nervous during the last period, with weakness in arms. Also choking sensation in throat. Slept very lightly and brokenly for about 7 hrs. Extremely nervous all night.

Feb. 28, 6 gr. sugar. Have felt some better to-day than yesterday but not quite normal. A little nervous all day. Slept better than previous night, but not so well as usual, for about 7 hrs.

March 1, 6 gr. caffein. Some pain in back of head during early morning. Very nervous from 12:00 to 1:00 with choking sensation in throat. Felt better at the 3:00 o'clock hour but a little nervous during the latter part of the day, due, I think, to an attack of nervousness on the part of another subject. Retired at 10:30, but was so nervous and wide awake that I could not sleep until after midnight. Tense feeling with some pain in back of head. Slept fairly well for about 6.5 hrs.

March 2, sugar. Some pain in the back of my head during the earlier morning hours, but have felt as well as usual during the rest of the day. (—).

General Result.—The prominent symptoms mentioned by this subject are more pronounced on caffein days than on control days, but curiously occur as often and as markedly before the dose was taken as later. The symptoms that seem to characterize the periods after the caffein dose are choking sensation, hot head and cold extremities, nervousness and weakness in arms, which symptoms do not especially characterize the post-caffein period until the 4 and 6 gr. amounts are reached. Sleep disturbance is not clearly present until after the 6 gr. dose is given, where it is apparent.

Subject 10, M., 28, 157, Occasional User, Squad IV

Feb. 10, 1 gr. sugar, mid afternoon. Felt as usual all day. Slept worse than usual for no apparent reason. 6 hrs. sleep.

Feb. 11, 1 gr. caffein, as above. Felt as usual. Slept better than usual. 8.5 hrs.

Feb. 12, 1 gr. sugar. Felt and slept as usual. 7.5 hrs.

Feb. 13, 1 gr. caffein. Felt and slept as usual. 7.5 hrs.

Feb. 14, 2 gr. sugar. Felt as usual. Slept better than usual. 8 hrs.

Feb. 15, 2 gr. caffein. Felt and slept as usual. 7 hrs.

Feb. 16, 2 gr. sugar. Felt and slept as usual. 7.5 hrs.

Feb. 17, 2 gr. caffein. Everything as usual. 7 hrs.

Feb. 18, 3 gr. sugar. Everything as usual. 8 hrs.

Feb. 19, 3 gr. caffein. Everything as usual. 7.5 hrs.

Feb. 20, 3 gr. sugar. Felt as usual. Slept poorly for 6 hrs.

Feb. 21, 3 gr. caffein. Felt as usual. Slept worse than usual, 5.5 hrs.

Feb. 22, 4 gr. sugar. Everything as usual. Slept 8 hrs.

Feb. 23, 4 gr. caffein. Felt worse than usual since 2:00 P.M. Have had a dizziness in the head and arm is quite nervous. Slept about as usual for 7 hrs.

Feb. 24, 4 gr. sugar. Everything as usual. Slept 8 hrs.

Feb. 25, 4 gr. caffein. Felt worse than usual. Experienced nervousness and dizziness, which began about 2:15 P.M. Slept as usual, 7.5 hrs.

Feb. 26, 6 gr. sugar. Felt and slept as usual, 7 hrs.

Feb. 27, 6 gr. caffein. Feel worse than usual. Have had a severe headache since about 3:00 P.M. Slept worse than usual on account of headache. 3 hrs.

Feb. 28, 6 gr. sugar. Felt and slept about as usual. 7 hrs.

March 1, 6 gr. caffein. Felt as usual. Never slept any. I know no cause, just sleepless.

March 2, sugar. About as usual.

General Result.—The introspective reports show no caffein influence up to the time of the 4 gr. doses. At this point nervousness and dizziness are reported. No sleep impairment. After 6 gr. caffein doses headache is reported and there is marked sleep impairment.

Subject 11, F., 27, 110, Abstainer, Squad IV

Feb. 10, 1 gr. sugar, mid-afternoon. Subdued headache till after lunch. Rest of day as usual. Slept 8.5 hrs., more soundly than usual.

Feb. 11, 1 gr. caffein, as above. Felt as usual till afternoon. Had slight headache, felt irritable but frisky. No desire to rest as I usually do for half hour after 1:00 o'clock. Slept as usual. 8 hrs.

Feb. 12, 1 gr. sugar. Felt as usual all day. Slept as usual. 8 hrs.

Feb. 13, 1 gr. caffein. As usual all day except for slight headache in early afternoon. Found it hard to sleep. Lay awake for not less than 1 hour. Awoke alert but tired. About 7 hrs.

Feb. 14, 2 gr. sugar. As usual all day except for sleepiness. Slept as usual, for about 8.5 hrs.

Feb. 15, 2 gr. caffein. Felt as usual till 2:00 P.M. Felt then confused and faint a few minutes, then the feeling passed away. But had intermittent returns followed by palpitation of heart during afternoon. Sensations such as follow shock of some sort. Slept 9.5 hrs. deep, dull sleep, not as usual, felt ill all evening, was feverish and shaky, had sensations of sinking during the night.

Feb. 16, 2 gr. sugar. Felt weak and ill all morning. Afternoon better, but still shaky. Some palpitation of heart and dull headache after 2:00 P.M. Severe headache in evening. Slept more soundly than usual, for 9 hrs.

Feb. 17, 2 gr. caffein. Felt somewhat duller than usual during the morning. Headache gone. Especially brisk about 3:00 P.M. Sinking sensations mildly felt about 5:00. Seemed to be intermittent. As well as usual in evening. Slept as well as usual, 8 hrs.

Feb. 18, 3 gr. sugar. Felt and slept as usual. 8 hrs.

Feb. 19, 3 gr. caffein. Felt shaky about 3:00 P.M. This continued off and on until 5:00. No feeling of illness. As usual during evening. Slept as well as usual for 8 hrs.

Feb. 20, 3 gr. sugar. Felt as usual all day. Slept better than usual, 9 hrs.

Feb. 21, 3 gr. caffein. Felt as usual till afternoon. Was confused from 2:00 to 3:00. Noticed some muscular trembling during afternoon, slept poorly for about 7 hrs. Exciting dreams.

Feb. 22, 4 gr. sugar. As usual till noon. From about 1:30 to 3:00 felt excitable. Heart beat very fast. Felt tired after 4:00, but otherwise as usual. Headache during evening. Slept more soundly than usual. 8 hrs.

Feb. 23, 4 gr. caffein. Felt as usual all morning. Very drowsy about 1:00 and unusually stupid. Went out of doors about 2:00 and felt as usual till the 3:10 test, then noticed unusual alertness. Continual overflow of spirits till 4:00. Then I noticed rapid heart beating which felt uncomfortable. Sudden perspiration followed by depression and apprehension. Felt shaky at 5:30, but as usual otherwise, and no sensations of illness or discomfort. Felt very irritable all evening. Calculated on 8 hours' sleep, but was wakeful and slept more lightly than usual, waking often.

Feb. 24, 4 gr. sugar. Morning headache. Felt tired and irritable (worse than usual). As usual all rest of day, except for slight headache in early afternoon and unusual sleepiness. Slept 8 hrs. more soundly than usual.

Feb. 25, 4 gr. caffein. Felt as usual till about 11:00 o'clock. Suddenly felt dizzy and stupid. Was decidedly dizzy for over an hour. Had no ambition to move or think till about 2:00. Gradual rise of spirits till 4:00, then a period of exuberance, of good feeling. Fanciful ideas rampant. Had three sudden attacks of perspiration. Gradual decrease of exhilaration but continued sensations such as felt after shock. Trembling of knees and hands. Uncertainty as to truth of ideas, so feel cautious. Slept as usual for 8 hrs.

Feb. 26, 6 gr. sugar. Felt as usual except for faint headache and dullness. Dull feeling passed away about 4:00 P.M. Severe headache in evening. Could not sleep first part of night and was wakeful during the remainder.

Feb. 27, 6 gr. caffein. Felt somewhat dull and had mild headache until about 3:00 P.M. Was greatly elated about 3:30. After 4:00 felt quivering of muscles in arms and shaking of knees. Sudden perspiration once. Mental confusion noticed from 4:00 to 5:00, after that sharp headache. Violent headache after dinner. Excitement subsided and was followed by deep mental depression and lack of energy. Slept as usual for 8 hrs.

Feb. 28, 6 gr. sugar. Felt as usual till about noon, after which I had a dull headache which continued till about 6:00 P.M. Was unusually stupid during evening. Slept 8.5 hrs., as usual.

March 1, 6 gr. caffein. Felt stupid all morning but otherwise as usual. Was exceedingly sleepy from noon till about 1:30. Had dull headache from 11:00 to about 2:00. Began to feel excited about 2:30 and increasingly happy for an hour. Gradual decrease of excitement after 4:00 o'clock. Felt only slightly shaky at 5:30.

March 2, (—) Was excused from the day's work.

General Result.—The symptoms headache, sleepiness in day and sleeplessness at night, excitableness and heart palpitation, alternating irritability and dullness, mentioned so frequently in this report are on the whole as characteristic of sugar days as of caffein days. (See February 10, 14, 16, 22, 24, 26, and 28.) It is however true that the minutely introspective tendency found in the part of the report given here does not characterize the daily records of the first week of the experiment during which only sugar doses were administered, although restlessness, frantic dreams and daytime lassitude are mentioned. But that these symptoms were more pronounced

on caffein days is obvious from the greater detail found in the reports on those days and from the greater amount of space taken for their description.

The attacks of perspiration, the dizziness, muscular quivering, the feeling of briskness and exhilaration, and the feelings of apprehension and distrust seem to be peculiar to caffein days only, and are present after doses of 2 gr. and over.

Subject 16, M., 24, 174, Regular User

Feb. 10, 1 gr. sugar, mid-afternoon. As usual except for little cold in head. Slept better than usual for 7.25 hrs.

Feb. 11, 1 gr. caffein, as above. Same as usual except for cold. Slept better than usual, 7 hrs.

Feb. 12, 1 gr. sugar. Felt all right until within an hour, when I felt slightly ''dippy,'' good deal the same ''weakish'' feeling, as if I had smoked too long. 6.75 hours' sleep, better than usual.

Feb. 13, 1 gr. caffein. All right except cold in head. Sleep better than usual, 6.75 hrs.

Feb. 14, 2 gr. sugar. Normal. Sleep (7.75 hrs.) poorer than usual.

Feb. 15, 2 gr. caffein. Normal. A little stale mentally. Sleep better than usual, 7.5 hrs.

Feb. 16, 2 gr. sugar. Felt worse than usual. Sleep better than usual, for 7.75 hrs.

Feb. 17, 2 gr. caffein. All right. In fact buoyant until 5:30, since which time I have had dull headache. 7.75 hours' sleep, better than usual.

Feb. 18, 3 gr. sugar. Same as usual. Head somewhat dull in latter part of day. 7.75 hours' sleep, good as usual.

Feb. 19, 3 gr. caffein. As usual in morning. Headache between 2:30 and 3:30. 7 hours' sleep, poor, a herd of nightmares.

Feb. 20, 3 gr. sugar. Duller than usual. Headache late in P.M. Stomachache all A.M. Sleep better than usual, 7.5 hrs.

Feb. 21, 3 gr. caffein. As usual to-day. Sleep (7.5 hrs.) better than usual.

Feb. 22, 4 gr. sugar. Dull headache, particularly hard in P.M. Sleep 6.5 hrs. better than usual.

Feb. 23, 4 gr. caffein. As usual this A.M. Better than usual from 2:00 to 3:00 P.M. From then until 5:00 nervous tremors as at end of race. Sleep better than usual, 7.5 hrs.

Feb. 24, 4 gr. sugar. Everything better than usual. 7.5 hrs.

Feb. 25, 4 gr. caffein. Better than usual. Shaky the last half hour. 7.5 hours, better than usual.

Feb. 26, 6 gr. sugar. As usual until late this P.M., when head ached for a couple of hours. 9 hours' sleep, worse than usual.

Feb. 27, 6 gr. caffein. As usual until 4:00 P.M., when better than usual. But at 5:30 felt sickish and decidedly shaky all over. Sleep worse than usual, for 7.25 hrs.

Feb. 28, 6 gr. sugar. As usual. Have a cough. 7.5 hours' ordinary sleep.

March 1, 6 gr. caffein. As usual this A.M. But have been bothered lately with indigestion. Felt stimulated this P.M. Had nervous tremors from head to foot. Some drowsiness. 7 hours' sleep, worse than usual. Nightmares. Awakened at 1:15.

March 2, sugar. As usual. Cough still present.

General Result.—No difference between caffein days and control days up to time of 4 gr. doses. After 4 and 6 gr. doses of caffein shakiness and tremor present. No signs of sleep disturbance for amounts less than 6 gr., after which, sleeplessness.

Subject 8, M., 24, 144, Regular User, Squad II

Feb. 10, 1 gr. sugar, 10:30 A.M. Feeling as usual, except for a slight headache, which has been felt from time to time all day. Slept as usual for about 8 hrs.

Feb. 11, 1 gr. sugar, as above. Feeling as usual. Sleep not quite as good as usual. 8 hrs.

Feb. 12, 1 gr. caffein, as above. As usual, except for slight headache, which may be due to a cold. Slept as usual for 7.5 hrs.

Feb. 13, 1 gr. caffein. As usual, except for headache just before lunch. Did not sleep as well as usual. 7 hrs.

Feb. 14, 1 gr. caffein. As usual, except for slight headache at close of day. Did not sleep quite as well as usual. About 7.5 hrs.

Feb. 15, 2 gr. sugar. Feeling as usual. Did not sleep as well as usual. About 6.5 hrs.

Feb. 16, 2 gr. sugar. Feeling as usual, except for slight headache just before noon. Slept as usual, for about 7.5 hrs.

Feb. 17, 2 gr. caffein. As usual, except for slight headache between 12:00 and 1:00 o'clock. Slept as usual. 7.5 hrs.

Feb. 18, 2 gr. caffein. Felt as usual. Did not sleep as well as usual. I awakened several times during the night. About 7 hrs.

Feb. 19, 2 gr. caffein. Felt and slept as usual. 8 hrs.

Feb. 20, 3 gr. sugar. Felt and slept as usual. 7.5 hrs.

Feb. 21, 3 gr. sugar. Felt and slept as usual. 7.5 hrs.

Feb. 22, 3 gr. sugar. Felt and slept as usual. 8 hrs.

Feb. 23, 4 gr. caffein. As usual, except for a headache which has recurred from time to time since about noon. Slept not quite so well as usual, for about 7.5 hrs.

Feb. 24, 4 gr. caffein. As usual, except for very *slight* headache during the early afternoon for about 3 hrs. Did not sleep as well as usual. About 6 hrs.

Feb. 25, 4 gr. caffein. Feeling as usual to-day, except that I have felt a trifle stupid. Slept as well as usual for about 8 hrs.

Feb. 26, 6 gr. sugar. Felt and slept as usual, about 8 hrs.

Feb. 27, 6 gr. sugar. Feeling as usual, except for slight headache between 12:00 and 1:00 and after 5:00 o'clock. Slept as usual, for about 7.5 hrs.

Feb. 28, 6 gr. sugar. Felt and slept as usual. About 8 hrs.

March 1, 6 gr. caffein. Felt as usual until about noon, when I began to feel a dull headache which lasted until about 1:00 o'clock. This returned about 3:00 and has remained ever since. I have been extremely nervous all afternoon and have felt feverish. Did not sleep as well as usual. About 7 hrs.

March 2, sugar. Felt as usual to-day, except for a headache since 3:00 P.M. Headache was not so bad as yesterday.

General Result.—The headache reported by this subject on so many days seems to be chronic, and is present as frequently on control days as on caffein days. (See February 10, 16, 27, and March

2.) But on caffein days the headache is reported as continuing until later in the day than usual. Signs of unusually poor sleep after the 4 and 6 gr. caffein doses. Nervousness and feverishness after the 6 gr. amount only.

Subject 12, M., 24, 160, Regular User, Squad II

Feb. 10, 1 gr. sugar, 10:30 A.M. Felt as usual, slept worse than usual for 7.5 hrs.

Feb. 11, 1 gr. sugar, as above. Felt as usual, slept worse than usual for 6.5 hrs.

Feb. 12, 1 gr. caffein. Felt and slept as usual, 7.5 hrs.

Feb. 13, 1 gr. caffein. Felt and slept as usual, 7.5 hrs.

Feb. 14, 1 gr. caffein. Felt and slept as usual, 7.5 hrs.

Feb. 15, 2 gr. sugar. Felt as usual. Slept better than usual, 7.5 hrs.

Feb. 16, 2 gr. sugar. Had bad headache during day, which was probably due to a bad cold. Especially bad about 1:00 to 4:00 o'clock, but somewhat better by 5:30. Slept as usual, for 7.5 hrs.

Feb. 17, 2 gr. caffein. Slight headache all day, especially bad at 5:30. May have been due to quinine, which was prescribed for my cold. Slept as well as usual, for 7 hrs.

Feb. 18, 2 gr. caffein. About as usual all day, though very nervous at the 12:00 o'clock session. Cause unknown. Slept as usual, for 7 hrs.

Feb. 19, 2 gr. caffein. Felt and slept as usual. 7 hrs.

Feb. 20, 3 gr. sugar. Felt about as usual all day, though better than for the past few days. Am beginning to recover from a cold and have not had any headache such as has been bothering me for the last few days. Slept as usual. 8 hrs.

Feb. 21, 3 gr. sugar. Felt and slept as usual. 7.5 hrs.

Feb. 22, 3 gr. sugar. Felt like a ''bone head'' all day. My head was dull more than usual. Otherwise all right. Slept as usual, 7.5 hrs.

Feb. 23, 4 gr. caffein. Felt as usual until about 12:00 o'clock. Then I became very nervous for some reason and had great trouble controlling my nerves. This nervousness continued until about 4:30, at which time I began to feel better. By 6:00 I felt almost as well as usual. Cause unknown to me. Slept as usual, 7 hrs.

Feb. 24, 4 gr. caffein. Felt as usual, except at 12:00, when I had a headache, and at 5:30, when I was nervous. Slept as usual, 7.5 hrs.

Feb. 25, 4 gr. caffein. Felt as usual until 12:00, when I became quite nervous and remained so until 6:30. Also had slight headache during the same period. Slept as usual. 6.5 hrs.

Feb. 26, 6 gr. sugar. Felt ''dippy'' and dull all day, cause unknown, unless weather is to blame. Slept as usual, for 7.5 hrs.

Feb. 27, 6 gr. sugar. Felt better when I got up than usual. Remained so all day. Slept as usual, 7.5 hrs.

Feb. 28, 6 gr. sugar. Felt as usual all day, though I was a little nervous about 5:30. Slept as usual, for 7.5 hrs.

March 1, 6 gr. caffein. Felt as usual until 12:00 o'clock, when I became extremely nervous and remained so until 6:00 P.M. During the same time I had also a fever. Slept worse than usual, 7.5 hrs.

March 2, sugar. Got up feeling worse than usual. Felt dull all day and had a bad headache.

General Result.—No clear difference between control and caffein days up to time of 4 gr. doses. Nervousness and headache follow about 2 hours after the 4 and 6 gr. caffein amounts. No sleep impairment except perhaps after 6 gr. dose of caffein.

Subject 13, M., 22, 175, Regular User, Squad II

Feb. 10, 1 gr. sugar, 10:30 A.M. Felt and slept as usual, 7 hrs.

Feb. 11, 1 gr. sugar, as above. Felt and slept as usual, but awoke with a terrific headache.

Feb. 12, 1 gr. caffein. Felt miserable this morning, with sick headache. Was prescribed calomel. Felt weak and indifferent after this dose. Slept as usual. 7 hrs.

Feb. 13, 1 gr. caffein. Felt very well all day, except for most uncomfortable rumbling and confusion which seems to be going on inside. Slept as well as usual. 7.25 hrs.

Feb. 14, 1 gr. caffein. Felt and slept as usual. 7.25 hrs.

Feb. 15, 2 gr. sugar. Felt and slept as usual. 8.75 hrs.

Feb. 16, 2 gr. sugar. Felt as usual to-day, with exception of slight heaviness this morning. I attribute this to too much sleep last night. Slept more heavily than usual, 6.75 hrs.

Feb. 17, 2 gr. caffein. Felt and slept as usual. 7.25 hrs.

Feb. 18, 2 gr. caffein. Felt and slept as usual. 6.75 hrs.

Feb. 19, 2 gr. caffein. Felt as usual all day with the exception of my eyes. Had them examined and they have been suffering a good deal. This morning I had a kind of headache. Broken sleep and headache in evening. Retired at 8:00 and arose at 6:45.

Feb. 20, 3 gr. sugar. As usual all day. Felt sleepy after dinner at night and retired at 8:30, arising at 6:45. Felt stupid and drowsy.

Feb. 21, 3 gr. sugar. As usual all day. Had a broken sleep from 9:00 to 6:45.

Feb. 22, 3 gr. sugar. Felt a little stupid, otherwise all right. Slept as usual, but only 6.5 hrs.

Feb. 23, 4 gr. caffein. Felt tremendously exhilarated and energetic for about an hour and a half, from 12:00 to 2:00, and since I have been extremely nervous and a trifle unstrung. Took a long time for me to go to sleep, slept as usual then, for 6.75 hrs.

Feb. 24, 4 gr. caffein. Felt as usual all day. Retired at 10:00 with just the beginnings of a cold. Took quinine and hot lemonade, and had a heavy sweat. Slept as usual and feel bully this morning.

Feb. 25, 4 gr. caffein. Felt as usual until about 1:00 P.M. and then began to feel very nervous. Felt nervous until 3:00 and had griping pains in abdomen. Still slightly nervous (6:30). Slept as well as usual, for 7:75 hrs.

Feb. 26, 6 gr. sugar. As usual all day. Slept more heavily than usual, for 7.75 hrs.

Feb. 27, 6 gr. sugar. As usual all day. Played three games of handball and felt extremely tired up till about 3:00. No pains or nervousness. Slept as usual, for 8.5 hrs.

Feb. 28, 6 gr. sugar. As usual all day. Slept as usual, but with queer and very unusual dreams. 7.5 hrs.

March 1, 6 gr. caffein. Felt as usual up to 12:00 or so. Felt extremely nervous, shaky physically, ever since. Not so nervous at time of writing (6:30), but very irritable. Slept as well as usual, for 6.75 hrs.

March 2, sugar. Feel miserable. Developed a sick bilious headache around 1:00 P.M. and it has been increasing in intensity steadily. No relief by 6:30 P.M.

General Result.—No effect up to time of 4 gr. doses of caffein. Nervousness and irritability follow these and the 6 gr. dose. No report whatever of sleep impairment after caffein. Stomach pains mentioned after one of the 4 gr. doses.

Subject 2, F., 38, Regular User. Worked Alone

Feb. 10, sugar, 8:30 A.M. Been dreaming of typewriting. Recovering from a cold. Feel tired and have headache and pain or ache in upper part of spine. Slept as usual. Strange dreams. 7.5 hrs.

Feb. 11, sugar, as above. Seem to be more nervous than usual. Did not have good control. Cold better. Not so much aching and not so tired. Slept about as usual, perhaps a little more wakeful. 7 hrs.

Feb. 12, sugar. Woke feeling well, except for cold, which is better. Felt the best physically of all days so far. Head felt a little full during the 3:10 period. Slept as usual, for about 7 hrs.

Feb. 13, 1 gr. caffein, as above. Felt best of any day. Cold about same. Did best work yet. Slept unusually well for about 7 hrs.

Feb. 14, 1 gr. caffein. Felt well all day, but tired at night and a little discouraged. Slept fairly well for about 7 hrs.

Feb. 15, 2 gr. sugar. Felt tired and languid all day, with slight headache, increasing a little toward night. Slept as usual, 7 hrs.

Feb. 16, 2 gr. caffein. Slight headache in morning disappeared by noon. Aside from cold, which is better than yesterday, have felt very well. Slept about as usual for 7.5 hrs.

Feb. 17, 2 gr. sugar. Felt some dullness and lassitude as is usual on such dark rainy days. Otherwise have felt very well. Slept as usual. About 7.5 hrs.

Feb. 18, 3 gr. caffein. Felt pretty well this A.M.; slight headache developing towards night (catarrhal) probably due to the damp weather. Slept very soundly for about 6 hrs., and less soundly for 1 hour more.

Feb. 19, 3 gr. sugar. Felt pretty well through the day, except that the work seemed to be an effort. Tired and somewhat discouraged when night came. Slept very poorly for about 6 hrs. Unusually nervous and worried.

Feb. 20, 3 gr. caffein. Have felt dispirited and out of harmony somewhat all day—perhaps due to lack of sleep—otherwise well. Slept about as usual for 7.5 hrs.

Feb. 21, 4 gr. sugar. Did not feel very well through the day, but better to-night. Slept unusually well for about 8 hrs.

Feb. 22, 4 gr. caffein. Felt fine all day, as is usual in good weather. Slept as well as usual for about 7 hrs.

Feb. 23, 4 gr. sugar. Felt as usual all day. Slept as usual, for 7.5 hrs.

Feb. 24, 4 gr. caffein. Felt well all day. Did not sleep nearly so well as usual, not more than 3 or 4 hrs. Seemed to be nervous. Heart thumping part of the time and then very faintly.

Feb. 25, 4 gr. sugar. Felt as well as usual. Slept as usual for 7 hrs. Felt tired in the morning, probably due to lack of sleep the night before and to the damp cloudy day.

Feb. 26, 4 gr. caffein. Slight headache most of the day, with some aching ii back. About as usual in soft damp weather. Slept fairly well for about 6 hrs.

Feb. 27, 6 gr. sugar. Felt about as usual, except tired from lack of sleep. Slept pretty well for about 7 hrs. Awoke with headache, due perhaps to cold.

Feb. 28, 6 gr. caffein. Headache most of day. Eyes felt weak and watery. Hands cold most of the day. Felt better toward night. Slept rather brokenly for about 7 hrs.

March 1, 6 gr. sugar. Felt cold all day, arms stiff, could not relax much. Slept very well for about 7 hrs.

March 2, 6 gr. caffein. Felt well most of the day. Slight headache part of the forenoon. (No record of sleep for this day.)

General Result.—Up to the time of 4 gr. doses there is no clear difference between caffein days and control days. After doses larger than this, headache is present, and disturbed sleep. Headache, nervousness, tired and discouraged feeling about as prominent on control days as on caffein days.

Final Statement of Results for All Squads

Allowance must be made for the tendency to headache and nervousness reported even by members of the control squad (see especially subjects 1, 7 and 15). The strain involved in the repeated completion of the series of tests at the highest possible level of performance was considerable. Thus the cancellation test produced such great eye strain on the part of one member of the control squad that it was found necessary to excuse him permanently from this test. Most of the subjects reported more or less strain directly traceable to the strenuous character of the tests themselves. Consequently only such symptoms can be securely taken to indicate caffein effect as are clearly present on caffein days only or are unusually prominent on those days, as compared with the control days. Tendency to headache, nervousness, dizziness, feverishness and occasional sleeplessness are distributed in a fairly uniform way throughout the reports of the control squad. Bearing these facts in mind, the following seems to be a fair statement of the influence of caffein on the general health and feeling of well-being in the case of the subjects participating in the present experiment.

Squad II. (weights 144, 160 and 175), taking pure caffein alkaloid doses three days in succession at 10:30 A.M., *without food-substance.*—No effect up to the time of the 4 gr. doses. After 4 and 6 gr. amounts, nervousness, feverishness, headache, irritability and disturbed sleep. Much the same thing may be said of Subject 2, who took doses at 8:30 A.M., working independently of the squads.

Squad III. (weights 130, 159, 193), taking pure caffein alkaloid on alternate days, with increasing doses, with the mid-day lunch.—No sleep impairment except in the case of the woman after 6 gr. Nervousness and heartburn or stomach pains after doses of 3 gr. or over. Dizziness and headache after 6 gr. amounts.

Squad IV. taking the doses on alternate days in the mid-afternoon on an empty stomach.—Men subjects (weights 157, 174), no influence up to time of the 4 gr. doses. For larger doses, nervousness and dizziness or headache. Sleeplessness after 6 gr. amounts only.

Women subjects (weights 105, 125, 110), dizziness or light-headedness, attacks of perspiration, numbness or coldness of extremities, nervousness, drawn feeling in throat and head, and sleeplessness unusually prominent in the case of *the two slightest subjects* after doses larger than 2 or 3 gr. In the case of the heaviest of the three, the symptoms do not appear in any unusual degree until after the 4 or 6 gr. amounts, as in the case of the men. The apparent sex difference found with this squad is probably entirely a function of body weight.

The subjects quite uniformly report improvement in health, spirits and general efficiency at the close of the experiment. This is perhaps due to the regular régime of life followed during the 40 days. Those who had given up the use of caffein-containing beverages during the experiment and for several days previous to its beginning do not report any craving for the drinks as such, but several expressed a feeling of annoyance at not having some sort of a warm drink for breakfast. Two subjects report a gain in weight, two a loss, and the rest either report no change or are unable to state.

The two principal factors which seem to modify the degree of the caffein influence are *body weight* and the *presence of food* in the stomach at the time of the dose. For more detailed study of the influence of caffein on the quality and amount of sleep, see the special chapter dealing with those points. There is a close correlation between the two sets of results (sleep and general health).

CHAPTER XV

CONCLUSION

THE results for each test have been briefly summarized at the close of the chapters. No attempt need be made to restate these conclusions here except perhaps by way of a schematic review of all the tests. Such a review is presented in the following tabular summary. Such a summary is of course wholly inadequate to express the significant facts which the various chapters have brought forward. Its chief interest comes from the assemblage of the various tests in groups, the groups being roughly designated by the psychological process or function which is especially prominent in the performance of the tests included in the group. It is clear at once that the caffein influences all the tests in a given group in much the same way. The effect on motor processes comes quickly and is transient. The effect on higher mental processes comes more slowly and is more persistent. Whether this result is due to quicker reaction on the part of motor nerve centers, or whether it is due to a direct peripheral effect on the muscle tissue, the pure psychologist can hardly be expected to know. Physiological experiment, however, seems to indicate that caffein has a direct effect on the muscle tissue, and that this effect is fairly rapid in appearance. The physiology of absorption also explains the fact that the presence of food substance in the stomach retards and reduces the caffein influence. The dependence of the amount of the caffein influence on the body weight of the individual has already been explained in terms of the amount of the substance ingested per unit of tissue affected.

One of the most interesting facts shown by these experiments is the complete absence of any traces of secondary depression or of any sort of secondary reaction consequent upon the stimulation which is so strikingly present in many of the tests. Rivers' conclusion, already referred to, that "caffein increases the capacity for both muscular and mental work, . . . without there being any evidence, with moderate doses, of reaction leading to diminished capacity for work," is thoroughly confirmed by the results of all the present experiments. This result is quite in contrast with the secondary reaction said to follow stimulation by such a drug as strychnine. It must be said that our present knowledge concerning the precise mode

of action of drugs on nervous tissue is very inadequate. That the increased capacity for work is produced is clearly demonstrated. That this result is a genuine drug effect, and not merely the effect of excitement, interest, sensory stimulation, expectation or suggestion, the carefully controlled tests here reported prove beyond any possible doubt. But whether this increased capacity comes from a new supply of energy introduced or rendered available by the drug action, or whether energy already available comes to be employed

SCHEMATIC SUMMARY OF ALL RESULTS

St. = Stimulation. 0 = No effect. Ret. = Retardation

Process	Tests	Primary Effect: Small Doses	Primary Effect: Medium Doses	Primary Effect: Large Doses	Secondary Reaction	Action Time, Hours	Duration in Hours
Motor speed	1. Tapping	St.	St.	St.	None	.75–1.5	2–4
Coordination	2. Three-hole	St.	0	Ret.	None	1–1.5	3–4
	3. Typewriting						
	(a) Speed	St.	0	Ret.	None	Results show only in	
	(b) Errors ...	Fewer for all doses.			None	total days' work.	
Association	4. Color-naming ..	St.	St.	St.	None	2–2.5	3–4
	5. Opposites	St.	St.	St.	None	2.5–3	Next day
	6. Calculation	St.	St.	St.	None	2.5	Next day
Choice	7. Discrimination-reaction time ..	Ret.	0	St.	None	2–4	Next day
	8. Cancellation ...	Ret.	?	St.	None	3–5	No data
	9. S-W illusion ...	0	0	0			
General	10. Steadiness	?	Unsteadiness		None	1–3	3–4
	11. Sleep quality ..	Individual differences depending on body weight and conditions of administration.					
	12. Sleep quantity .					2?	
	13. General health .						

more effectively, or whether the inhibition of secondary afferent impulses is eliminated, or whether fatigue sensations are weakened and the individual's standard of performance thereby raised, no one seems to know. The interpretation is obscure but the facts are plain.

The widespread consumption of caffeinic beverages under circumstances in which and by individuals for whom the use of other drugs is stringently prohibited or decried seems to be justified by the results of experiment. But it should be emphasized that the results of the investigation here reported bear only on the more or less immediate effects of caffein on performance. It is true that the investigation as a whole covered a period of 40 days, and that in the intensive experiment the effect of single doses was traced for a period of 3 days. But the results can not be carried over bodily to the

question of the continuous use of the drug. One can only assume that if the constant use of caffein in moderate amounts would prove deleterious, some indication of such effect would have shown itself in the careful study of performance in tests covering a wide range of mental and motor processes, a wide range of doses and of individuals, and of time and conditions of administration. Nor can anything be said, on the basis of these results, concerning the physiological or neurological effect of caffein, except in so far as integrity of structure can be inferred from unimpaired function or performance.

It should be further pointed out that the quantitative results of this investigation of the influence of caffein in its pure form can not be directly compared with the action of its citrated form, which is only half caffein, the remainder being citric acid which itself has a demonstrable action on nerve and muscle tissue. Much the same thing is true of the action of tea, coffee, and other caffeinic beverages, which contain a variety of other substances which may be supposed to enhance or neutralize or otherwise modify the effect of the caffein content.[1] Many of the results commonly attributed to these beverages undoubtedly come, in so far as they can be demonstrated at all under controlled conditions, from these non-caffein ingredients.

[1] The average cupful of hot tea (black, 5 fluid ounces) contains about 1.5 gr. of caffein. About the same amount is present in the average after-dinner cup of black coffee (2 fluid ounces). An average glass of cold green tea (8 ounces exclusive of ice) contains about 2 grains, while an average cupful of coffee with hot milk (5 ounces, three fifths coffee and the rest milk) contains about 2.5 grains of caffein.

Reaction Time To Retinal Stimulation

WITH SPECIAL REFERENCE TO THE TIME LOST IN CONDUCTION THROUGH NERVE CENTERS

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CONTENTS

INTRODUCTION

Since the introduction of the neurone theory in 1891, and the recognition of the cell-body as nutritive in function, attention has turned from the cell-body to the synapse, or the surface of separation between neurones, for an explanation of the processes peculiar to the nerve centers. It is not the physiologist alone who seeks in the synapse the cause of the peculiarities of central function, but the psychologist also is beginning to search for an explanation of the mental processes in the modification of conductivity in the synapses. Of the physiologists, Sherrington[1] may be mentioned, the originator of the most generally accepted theory of the process taking place at the synapse. In the field of psychology probably the most notable example of the appropriation of the synapse theory as the physiological basis of the central processes, is Pillsbury's "Essentials of Psychology." In it changes in the synapse form the physiological concomitant of learning in general, of instinctive movements, habit formation, attention, association and retention. To quote from Pillsbury:

> At present the tendency is to explain all learning as due to a change at the point where two neurones come together, where the end-brush of one comes into contact with the dendrites of another. This point, as was said, has been called the synapse (p. 49). Evidently then, the most important question of nervous physiology for the psychologist is what is the nature of the synapse and its action (p. 50)? Learning, whether of new movements or of new ideas, is a process of making easier the passage of an impulse from neurone to neurone, and is fundamentally the same everywhere (p. 52).

Concerning instinctive movements he says:

> The part of the nervous system to which we must look for an explanation is the synapse, the point of connection between nerve cells (p. 240). The formation of habits is thus a process of decreasing resistance in the synapses in the different possible paths of transmission (p. 55). The change in the synapse as a result of use is the explanation of association as of habit (p. 138).

In regard to retention and recall he says:

> It is probable that the point of connection between the nervous elements involved is where the change that predisposes for return occurs (p. 133).

The changes at the synapse are expressed by physiologists and psychologists alike as modifications of conductivity, lowered resist-

[1] See Sherrington, "Integrative Action of the Nervous System," 1906.

ance, openness of paths,. etc., changes which may be detected by a variation in the speed of conduction or the time lost in conduction over synapses. An eminent physiologist[2] considers it quite probable that in the same individual, under the same conditions, the time lost by the block at each synapse is about the same, and that a careful study of the differences in reaction time and of the time lost in the passage through nerve centers may help to form a conception of the amount of delay for which each synapse is responsible.

The experiment to be described is an attempt to develop a method of measuring the time lost in the transmission of an impulse through a synapse within the human nervous system, and to obtain an approximate measure of this lost time under controlled conditions. When such an estimate of the synapse time has been obtained, those theories which associate the various mental processes with a modification of synapse conduction may be put to an experimental test.

Chapters I. and II. of this study give a history of the inquiry into the speed of conduction through nerves and nerve centers, for the purpose of showing the methods used, the great variations in the results and the difficulties that have been encountered; Chapters III. and IV. outline the method and describe the apparatus used; Chapters V. and VI. consider the several preliminary problems which arise in connection with the methods adopted, but which have also a certain intrinsic interest; and Chapter VII. discusses the main problem of synapse time.

I gratefully acknowledge the help received from Professors J. McK. Cattell and R. S. Woodworth, under whose guidance the experimental work was conducted, and from Dr. E. K. Strong, Jr., research fellow in advertising, and John W. Todd, assistant in psychology, who have rendered very valuable assistance in numerous ways.

[2] See Schäfer, "Textbook of Physiol.," 609, 1900.

CHAPTER I

HISTORY OF THE INQUIRY INTO THE SPEED OF NERVE CONDUCTION

THE history of the inquiry into the speed of transmission of the nerve impulse seems to date from the very discovery of a nerve principle. The early idea of the nerve impulse as some vague and mysterious force gave rise to the conception that its speed must be comparable to that of light, or infinitely fast. Haller[1] in his "Elementa Physiologiæ," gives an account of some of these ideas and the methods of calculating the speed of nerve conduction. One reasoned that since the aorta is 2,880 million times the size of the most delicate nerve fibrils in the heart, the speed of the nerve fluid must be that many times the speed of the blood traveling through the aorta, or 57,600 million feet per second. Haller himself based his conclusions on the assumption that the speed of rhythmic movements was limited by the time required for an impulse to travel from the muscle to the brain and return. The total distance traversed and the time being known, he obtained a speed of 9,000 feet per minute.

The first real evidence against the supposed tremendous speed of nerve conduction came in 1795, from the astronomer Maskelyne, who noticed differences in the reports by different men of the same celestial occurrence. Bessel[2] found this difference to be a constant and called it the "personal equation." Nicolai[3] was probably the first to explain the personal equation by differences in the speed of the nervous process. Johannes Müller,[4] however, opposed this explanation of the personal equation as due to the differences in the speed of the nerve impulse and attributed it to purely psychological factors such as attention. In 1848 he stated that the speed of nerve conduction could never be calculated, since the speed was so great and the distances to be traversed in the human body were so short.

[1] Haller, "Elementa Physiologiæ," **4**, 372, 1762.

[2] For a general review of the work on the personal equation, see E. C. Sanford, "The Personal Equation," *Am. Jour. Psych.*, **2**, pp. 2, 271, 403 ff., 1888–1889.

[3] See Hermann, "Handbuch der Physiol.," **2**, 16, 1879.

[4] "Handbuch der Physiol.," **1**, 581, 1844.

I. Speed of Conduction in the Motor Nerves of Animals

Only about six years after this statement by Müller, Helmholtz[5] measured the speed of conduction in the sciatic nerve of the frog. He applied an electrical stimulus to the nerve at different distances from its connection with the muscle and compared the resulting differences in the time of the muscular response, with the length of nerve between the points stimulated.[6] He found an average time of 28 meters per second, with slight variations according to changes of temperature. During the next few years, other investigators confirmed these results, using the same general methods, although varying the means of recording the results. For instance, Fick,[7] in 1862 found similar results. Harless,[8] in the same year, used an adaptation of the Atwood machine, in which the falling body consisted of a smoked plate, on which the moment of stimulation and muscular contraction were directly recorded. Thiry,[9] in 1864, employed about the same methods as Helmholtz with a different means of regulating

[5] "Messungen über den zeitlichen Verlauf der Zuckung animalischer Muskeln und die Fortpflanzungsgeschwindigkeit der Reizung in den Nerven," *Archiv f. Anat. u. Physiol.*, 276–364, 1850.

[6] Helmholtz used two methods of measuring the speed of nerve conduction. The first method was an adaptation of the Pouillet'sche method of time measurement. The principle on which this method is based is that a magnet acting upon a needle will divert it a distance proportional to the time during which the current acts. The time is thus calculated from the distance the needle moves. One end of the muscle was attached to a permanent bracket, while the other was fastened to a lever in such a way that the slightest contraction of the muscle would move the lever and break the contact. The electric circuits were so arranged that when the electric stimulus was applied to the nerve, the current was sent through the magnet. As soon as the muscle contracted, this time circuit was broken and the time read off from the position of the compass needle. See Hermann, "Handb. der Physiol.," **1**, 31–33, 1879.

The second method made use of Helmholtz's myograph, a detailed description of which may be found in Hermann, "Handb. der Physiol.," **1**, 23 ff., 1879. The muscle was attached to a writing lever which wrote upon a revolving glass cylinder. The stimulus was given by an induction shock. A curve was recorded upon the glass cylinder when the stimulus was applied near to the muscle. It was then applied at a distance from the muscle and the second curve superposed upon the first. The distance between the beginning of each curve was then turned into a time value, from the known speed of the revolving drum. This measured the time of conduction over the stretch of nerve between the two points of stimulation. The shock was applied through a contact upon the edge of the drum, an arrangement which enabled the stimulus to be given at the same point in each revolution of the drum.

[7] "Vierteljahrschrift der Naturforschenden Gesellschaft in Zürich," 1862.

[8] *Abhand. der Bayer. Akademia der Wissenschaft*, Bd. **9**, Abth. 2, 316.

[9] *Zeitschr. f. rat. Med.*, Bd. **21**, 300, 1864.

the speed of his registering apparatus. According to Munk,[10] the excitation does not travel over the nerve at a constant speed. If one takes three points on a nerve fiber equally distant from each other, the speed of transmission between these points would not be the same, although the lengths of nerve would be equal. According to him, the excitation seems to take less time to travel from the upper extremity to the middle of the nerve than from the middle to the lower extremity.

Marey,[11] in 1868, by his own adaptation of the graphic method in which a revolving disc was used instead of a drum, found a speed of transmission of 11 meters per second upon electrical stimulation of the motor nerves of the frog. Lamansky,[12] by a slightly different method, found a speed of 31 meters per second. Bernstein[13] deviated from the previous methods in that he made use of the action current of nerve to register the time of reaction, instead of the contraction of muscle itself. He obtained a speed of 25 to 33 meters per second. Valentin[14] in 1870 published an experiment on frogs, in which he found a speed of nerve conduction in the gastrocnemius muscle of 14 to 20 meters per second. This result was valid only for a nerve-muscle preparation kept in very cold water. When a warmed preparation was used the speed increased to 25 to 30 meters per second. Chauveau,[15] in order to show that the results of experiments made on frogs' nerves are not applicable to mammals, made a comparative study upon the motor nerves of frogs and horses. The nerves in both cases were left in their normal position in the body. To prevent any disturbance of the results through voluntary movement, chloral was administered, or else the cord was severed at the bulb and the animal kept alive by artificial respiration. In the horse he worked upon the pneumogastric and recurrent nerves and in the frog upon the sciatic nerve. Unipolar electric stimulation was used and the results were graphically recorded. In the frog Chauveau obtained a speed of 21 meters per second, and the same method gave a speed of 65 meters in the horse. He also found individual differences in the speed of conduction varying from 40

[10] "Untersuchungen über die Leitung der Erregung im Nerven," *Archiv f. Anat. u. Physiol.*, Physiol. Abth., 798, 1860.

[11] "Du Mouvement dans les Fonctions de la Vie," 410–444, 1868.

[12] See Richet, "Physiol. des Muscles et des Nerfs," 376, 1882.

[13] See Hermann, "Handb. der Physiol.," **2**, 18, 1879.

[14] "Fortpflanzungsgeschwindigkeit der Nervenerregung," *Moleschott's Untersuch.*, **10**, 526 ff., 1870.

[15] "Vitesse de Propagation des Excitations dans le moteurs Nerfs des Muscles de la Vie animale, chez les Animaux mammiferes," *Compt. rend. Acad. de Sc. Par.*, **87**, 95–99, 138–142, 1878.

to 75 meters per second. Two further conclusions were drawn from his work: (1) If one increases the distance which separates the muscle from the point stimulated, the time taken for the conduction of the excitation increases less rapidly than the length of fiber traversed. (2) The excitations travel less rapidly as they approach the terminations of the nerve, that is, the conductivity decreases from the origin toward the termination of the nerve.

In 1880, Fredericq and Vandevelde[16] experimented upon the motor nerves of the lobster, using the graphic method of Helmholtz, and found an average speed of 6 to 12 meters per second. Ott,[17] in a study upon the "Rapidity of Transmission of Nerve Force in Normal and Stretched Nerves," obtained in the motor nerves of the frog, a speed ranging from 20 to 30 meters per second. His results showed that stretching a nerve lowered the speed at which it conducts impulses. Englemann[18] reported the speed of conduction in the nerves of the frog, upon mechanical stimulation at different points, to be 22.5 meters per second. He considers the differences found by other experimenters to be due to the condition of the nerve rather than to external conditions such as temperature and the like. By adopting certain refinements in method he was able to apply an electrical stimulus to points on a nerve only a short distance apart without spreading of the current. He concludes that the speed of conduction is the same in all parts of the nerve, except perhaps very near to its central and peripheral connections, and that the speed does not change upon a continued maximal stimulation.[19]

II. Speed of Conduction in the Motor Nerves of Man

Helmholtz and Baxt[20] applied the methods used by Helmholtz on frogs, to determine the speed of conduction in the motor nerves of man. By stimulating the median nerve near the wrist and in the upper arm, and recording the resulting contractions of the muscles of the thumb, they obtained an average speed of 34 meters per second. From their experiments they concluded that the speed of con-

[16] "Vitesse de Transmission de L'excitation motrice dans les Nerfs du Homard," *Compt. rend. Acad. d. Sc. Par.*, **91**, 239, 1880.

[17] *J. Nervous and Mental Disease*, **5**, 94–98, 1878.

[18] "Graphische Untersuchungen über die Fortpflanzungsgeschwindigkeit der Nervenerregung," *Archiv f. Anat. u. Physiol.*, s. 1, 1901.

[19] For the relation of the speed of nerve conduction to the intensity of the stimulus, and for references to work on the subject, see Von Wittich, *Pflüger's Archiv*, **2**, 329, 1869. Hermann, "Handbuch der Physiol.," **2**, 24, 1879. Nicolai, *Arch. f. Anat. u. Physiol.*, Suppl., 367, 1905.

[20] *Monatsber. der Berliner Acad.*, 228, 1867; 184, 1870.

duction varies in different parts of the same nerve. Thus the speed in the nerve in the upper arm was thought to be slower than that in the nerve in the forearm. Place,[21] by the same methods as those used by Helmholtz and Baxt, found very definite differences in different parts of the median nerve. For the forearm he obtained a speed of 62 meters per second and for the upper arm 17 to 24 meters per second. Later experiments by Helmholtz and Baxt gave higher values for the speed of nerve conduction, 65 meters per second, and contrary to their earlier conclusions and those of Place, showed that conduction was slower in the lower part of the median nerve than in the upper part. Oehl,[22] somewhat after the manner of Helmholtz, stimulated the radial nerve in the armpit and on the inner side of the index finger. He measured the resulting contractions of the finger and found the average time of conduction for the normal body temperature[23] to be about 30 meters per second. Alcock,[24] in 1895, performed an experiment on "The Rapidity of the Nervous Impulse in Tall and Short Individuals." By means of a writing lever pneumatically controlled, he measured the time between the twitches of the finger when the median nerve was stimulated at the shoulder and near the elbow. He obtained an average speed of 66.8 meters per second. He found that the size of the body had no influence on the speed of the nerve impulse. Since, therefore, the rate of transmission of an impulse is the same in all individuals, the time required for an impulse to traverse the limbs of a tall man is appreciably longer than in the case of a short man. Hermann[25] gives an average speed of 40 meters per second for the motor nerves of man.

Von Wittich[26] applied the reaction time method to the study of conduction in the motor nerves of man, and although his method differed greatly from that employed by the other experimenters, the results were about the same. He stimulated a certain point on the surface of the body and required a reaction by muscles located at different distances from the brain. He assumed that the sensory and central processes would remain the same, and that the resulting

[21] "Uber die Fortpflanzungsgeschwindigkeit des Reizes in den motorischen Nerven des Menschen," *Pflüger's Archiv*, **3**, 424 ff., 1870.

[22] "Nouvelles experiences touchant l'Influence de la Chaleur sur la Velocite de Transmission du Mouvement nerveux chez l'Homme," *Arch. ital. de Biologie*, t. **24**, 231, 1895.

[23] For the influence of temperature on the speed of nerve conduction and a bibliography, see Hermann, "Handbuch der Physiol.," **2**, 23 ff., 1879; and Oehl, *Arch. ital. de Biologie*, t. **24**, 231 ff., 1895.

[24] *Proceedings of the Royal Society of London*, **72**, 414–418, 1903.

[25] "Lehrbuch der Physiol.," s. 218.

[26] *Zeitschr. f. rat. Med.*, **31**, 106, 1868.

differences in time would be due to the differences in length of motor nerve traversed. He thus found an average speed of 30 meters per second.

III. Speed of Conduction in Sensory Nerves

Helmholtz[27] was the first to calculate the speed of conduction in the sensory nerves of man, by means of what was later called the "reaction time" method. Points on the skin at different distances from the brain were stimulated and as soon as the stimulus was felt, the subject reacted by breaking an electric circuit. The total reaction time included the latent time of the sensory end organ, the time required for the excitation to reach the brain, to pass through the brain centers, to travel over the motor nerve, and the latent time of the contracting muscle. Helmholtz assumed that all the factors except the length of the sensory nerve remained the same, hence the difference in time resulting from the stimulation at different points must be the time required for the stimulus to pass over the additional length of sensory nerve, included between these two points. By this means he found a speed of 60 meters per second.

Upon this same assumption and with methods varying only in the recording apparatus, the experiment has been repeated by many investigators with widely divergent results. Hirsch,[28] in 1861 using the Hipp chronoscope and applying his stimuli to the hand and foot, found a speed of about 34 meters per second. To the objection that the two paths could not be compared on account of the different lengths of spinal cord traversed, he replied that the speed of conduction was the same for white matter wherever found, and that the amount of gray matter was the same in both cases. Schelski[29] used the graphic method of recording his results and stimulated electrically the groin and the foot, thereby making the length of cord traversed equal in both cases. In this way he obtained an average speed of 94 meters per second. De Jaager,[30] using the same graphic method and the same points of stimulation as Schelski, found an average speed of 26 meters per second. Von Wittich[31] employed the graphic method and stimulated a great many points on

[27] See Hermann, "Handbuch der Physiol.," **2**, 18, 1879.

[28] "Chronoscopische Versuche über die Geschwindigkeit der verschiedenen Sinneseindrücke und der Nerven-Leitung," *Moleschotts Untersuch.*, **9**, 183, 1865.

[29] "Neue Messungen der Fortpflanzungsgeschwindigkeit des Reizes in den menschlichen Nerven," *Archiv f. Anat. u. Physiol.*, 151, 1864.

[30] See Hermann, "Handbuch der Physiol.," **2**, 20, 1879.

[31] "Uber die Fortleitungsgeschwindigkeit in menschlichen Nerven," *Zeitschr. f. rat. Med.*, **31**, 87 ff., 1868.

the skin at different distances from the brain, requiring a reaction by the hand. He found an average speed of 39 meters per second.

Marey[32] calculated the speed of conduction in the sensory nerves of the frog by means of reflex action. He cut the cord above the sciatic center and then stimulated the sensory nerve at different points and observed the differences in the latent time of the contraction of the opposite leg. In every case he obtained a speed greater than 30 meters per second. He injected strychnin into the frog and took his observations before tetanus set in, in order that the heightened irritability would bring about contractions of the opposite leg, that is, reflex contractions. The leg stimulated was bound so as to be immovable. Marey attributed the high speed obtained to the injection of the strychnin. In certain experiments[33] performed on himself according to the method of Helmholtz, he obtained a speed of 30 meters per second. Richet[34] criticized all the methods of finding the speed of conduction in the sensory nerves, which included in the paths used, various lengths of spinal cord. He selected for his own work two points on the arm, one at the shoulder and the other at the finger tips. From stimulation of these points he calculated a speed of 50 meters per second.

Bloch[35] devised an experiment by which the speed in the sensory nerves could be calculated independently of the motor nerves. He first showed that the method used by Schelski and others, of stimulating points at different distances from the brain and observing the resulting voluntary reactions, was invalid. He found that stimulating the finger brought forth a quicker reaction than stimulating the cheek, also that the general position of the body has an influence upon the speed of reaction. He attributed these differences not only to variation in the sensibility of the part stimulated, but also to differences in the speed of the volitional process. Thus an impulse from the hand could be more quickly transformed into a motor response than one from any other part of the body. His conclusion was that the speed of the sensory nerve current can not be studied when volition is a part of the process employed; also that when the sensibility of the parts is the same and the strength of the stimulus is uniform, the persistence of the sensation in consciousness is the same. By stimulating points at different distances from the brain, he found that the greater the distance between the points stimulated,

[32] ''Du Mouvement dans les Fonctions de la Vie,'' 410–444, 1868.

[33] ''La Machine animale,'' 43, 1873.

[34] ''Physiologie des Muscles et des Nerfs,'' 583, 1882.

[35] ''Experiences sur la Vitesse du Courant nerveux sensitif chez L'Homme,'' *Archiv de Physiol., norm. et path.*, 588–623, 1875.

the greater was the difference in the time of the appearance of the sensation in consciousness. He then timed the application of his stimuli in such a way that the two stimuli should be perceived at the same time, that is, that the two sensations should fuse. In order that this might take place, the point farther from the brain would have to be stimulated earlier than the nearer point. This time interval he considered to be due to the difference in the length of sensory nerve traversed. His results thus obtained were 192 meters per second in the nerves and 194 meters per second in the spinal cord. His method was based on the assumption that the fusion of two sensations in consciousness depends only upon the time of their arrival in the center and not at all upon their points of origin. His figures, however, suggest that the time of the appearance of a sensation in consciousness may vary according to the part of the body stimulated.

Hall and Kries,[36] in 1879, published an experiment on the "Dependence of Reaction Time upon the Place of Stimulation," and concluded that the central or reduced reaction time varies with the place stimulated, and that such variation is due not to a difference in the sensitivity of the parts (as shown by the fact that the reaction to a stimulus on the tongue is slower than to one on the forehead, although the former is about 20 times more sensitive), but it is due rather to the relative use or exercise of the parts. For instance, the reaction to a stimulus on the finger tips is faster than to one on the upper arm, although the distance traversed by the impulse is greater in the former case. Also the reaction to a light stimulus falling on the fovea centralis or the point of clearest vision, is more rapid than to one falling on the periphery of the retina. These differences in time the authors believed to be the result of differences in the speed of the central processes, which are faster for the finger and the center of vision. The speed of sensory conduction was for one subject found to be 214 meters per second, and for the other a negative value was obtained, that is, the point farther from the brain gave the quicker reaction. They concluded from their experiments that the speed of the central processes controlling movement increases with use, so that the speed of conduction in motor nerves calculated from the reaction by different members of the body is invalid. The speed of transmission in the cord, therefore, based on these erroneous figures, would be unreliable.

Hermann[37] concludes from the physiological identity of motor and sensory nerves, that the speed of conduction is the same in both

[36] *Archiv f. Anat. u. Physiol.*, Suppl., 1–10, 1879.

[37] "Handbuch der Physiol.," **2**, 23, 1879.

and that the apparent difference is due to the difference in the intensity of the stimuli used. Oehl, Fasola and Predigeri[38] used the reaction time experiment and stimulated points at different distances from the brain. They obtained an average speed for sensory nerves of 32 meters per second.

IV. More Recent Methods and Results

Reichert,[39] in 1888, experimented upon the nerves of rabbits in order to determine whether the nerve impulses travel at a rate directly proportional to the length of the fiber. This law holds for the nerve muscle preparation but it has been suggested that a different condition exists in the intact nerve and cord. Reichert experimented upon living animals, and in order to prevent volitional movements, the spinal cord was cut in the lower dorsal region. The stimulus was an induction shock applied to the nerve near its entrance into the muscle and at the pelvis. An independent electric circuit was so arranged that contraction of the muscle broke a metal contact, and this break in the circuit was recorded on a kymograph along with the stimulus and time record. An average of all his experiments gives a rate of transmission of 185 meters per second. In ten out of nineteen experiments the time required for the transmission of the impulse was longer by about 6.5 sigma when the point nearer the muscle was stimulated, than when a farther point was stimulated. These results are in accord with those of Munk and Chauveau, who found that the speed of conduction was reduced as the nerve termination is approached.

In 1893 Cattell and Dolley[40] made an extended research to determine the conditions which affect the length of reactions to dermal stimuli and to study the application of the reaction time experiment to the measurement of the velocity of the nervous impulse in motor and sensory nerves and in motor and sensory tracts of the cord. They employed electrical and tactile stimuli at points on the arm and leg over the median and posterior tibial nerve respectively. With electrical stimulation, the speed of conduction in the median nerve was for one subject 21 meters per second, and for the other 49 meters per second; in the tibial nerve 31 meters per second for the one subject and 65 meters per second for the other.

[38] "Sur la Velocité de Transmission de L'excitation dans les Fibres sensitive de L'homme," *Arch. ital. de Biologie*, 17, 400, 1892.

[39] "The Velocity of Nerve Impulses in Cut and Intact Nerves," *Jour. of Nervous and Mental Disease*, May, 1889.

[40] "On Reaction Times and the Velocity of the Nervous Impulse," *Memoirs of the Nat. Acad. of Sciences*, 7, 393 ff., 1893; *Psych. Rev.*, 1, 159, 1894.

When the stimulus was applied to the arm, the reaction by the foot was for *A*, 37.7 sigma, and for *B* 54.4 sigma slower than by the hand. When the stimulus was applied to the lower leg, reaction by the foot was for *A* only 29.7 and for *B* 45.0 sigma slower than by the hand. This means that when the stimulus is applied to the reacting member the time is shorter by 8.0 sigma for *A* and by 9.4 sigma for *B*, than when another member reacts. This difference may be attributed to the delay of coordination in the brain centers. The following figures give these results in tabular form:

Subject	Reaction	Stim. on Arm	Stim. on Leg	Diff.
A	By hand	163.8	190.7	
	By foot	201.5	220.4	
	Difference	37.7	29.7	*8.0*
B	By hand	119.2	147.3	
	By foot	173.6	192.3	
	Difference	54.4	45.0	*9.4*

On account of certain difficulties in the use of the electrical stimulus, a tactual stimulus was employed in later experiments. By this means the times for the upper and lower arm and upper and lower leg were practically the same. In one subject the reaction time upon stimulation of the finger was shorter than when the arm was stimulated, and somewhat shorter for the toe than for the thigh. These differences must clearly be due to the differences in the time of the cerebral reflex. The conclusions of Cattell and Dolley were as follows:

1. It does not follow that electrical stimulation of the excised and dying nerve of the frog produces the same effects as the cerebral discharge in the living animal, nor that these effects would hold for man.

2. The velocity of the normal nerve impulse can never be determined by electrical stimulation, owing to the great variety of results, which must be due to the methods of stimulation and not to the velocity of the normal impulse.

3. The chief difficulty in measuring the velocity of the sensory impulse is not the variable error of the cerebral reflex, but the fact that the same physical stimulus applied to different parts of the body produces physiological effects varying in intensity and cerebral discharges varying in facility.

4. The differences in the reaction time which occur in different individuals and in the same individual at different times are due, not to the differences in the length of nerve traversed, but to differ-

ences in the speed of the cerebral process. The reaction times for points nearer to the brain than others may be shorter because the physical effects of the shock may be greater or because the fibers from the upper points may lead to a more rapid transference in the brain.

5. The velocity of the nerve impulse is greater than that commonly supposed, 30 meters per second, and probably one half of the total time is taken up in the cerebral processes.[41]

DuBois Reymond[42] in 1900 published a paper on the speed of transmission of the nerve impulse in different parts of the nerve. He used the most approved and careful methods of stimulation and of recording the results. All of his work was done on the nerve muscle preparation of the frog. Three different parts of the nerve were selected and an average of seven series of experiments upon each region gave the following differences for stimulation of the limiting points of each region, 9.2, 9.4, and 9.2 sigma. He found that the greater the number of experiments the nearer the averages for the different parts of the nerve approached each other. He therefore concluded that the differences found in previous researches were due to irregularities in the methods and to insufficient data.

Kiesow[43] in 1903 devised a method of stimulating various points on the skin to avoid the possible influence of a difference of intensity upon the time of reaction. He marked out different touch spots on the arm and leg and tested these for their sensitivity. He then stimulated them by an electrically controlled touch hair, thereby making the physical stimulus uniform. The spots used in each experiment were on the same member and were of the same degree of sensitivity. He assumed that the cerebral processes would be equally rapid in such cases, and that the differences in reaction time would be due to the difference in the length of the sensory nerve traversed. His results showed a speed in the arm of 31 meters per second, and in the leg, 33 meters per second. He compared these figures with those of Helmholtz and Baxt and concluded that no distinction can be claimed between the speed in motor and sensory nerves of man

[41] "The Time taken up by the Cerebral Operations," *Mind*, London, **11**, 220–242, 1886.

[42] "Über die Geschwindigkeit des Nervenprincips," *Archiv f. Anat. u. Physiol.*, Physiol. Abth., Suppl., 68, 1900.

[43] "Zur Frage nach der Fortpflanzungsgeschwindigkeit der Erregung im sensiblen Nerven des Menschen," *Ztschr. f. Psychol. u. Physiol. des Sinnesorgane*, **33**, 444 ff., 1903.

Nicolai[44] experimented on the olfactory nerve of the pike and found no difference in the rate of transmission in different parts of the nerve. He also found that within the nerve trunk conduction was equally rapid in either direction.

The most recent work on nerve conduction is that on the motor nerves of man, by Piper[45] in 1908, who followed the method used by Helmholtz except in the manner of recording the results. Two points were selected for stimulation, one in the armpit and the other near the elbow, both being directly over the median nerve, and about 16 centimeters apart. An induced current was used for stimulation and the indifferent electrode was placed on the back of the subject. The time of the muscular response was not measured by its contraction, but by the production of the action current in the muscle. The muscle was connected to an Edelmann string galvanometer by means of electrodes placed upon the skin over the muscle. The movements of the galvanometer, together with a time record, were reflected upon a screen and photographed.[46] The galvanometer curve showed not only the beginning of the action current, but also the moment of stimulation, since a part of the stimulating current was conducted into the galvanometer circuit and caused a break in the curve, quite different from that produced by the action current. By these measurements he found the time interval greater, the nearer to its central end the nerve was stimulated. The average time lost in conduction over the 16 centimeters of nerve fiber was 1.36 sigma, giving a speed of 117 to 125 meters per second. By calculating the time of transmission from the lowest point stimulated to the muscle, a distance of 10 centimeters, according to these figures and subtracting the result from the measured time, he found the latent time of the muscle and the nerve end organs to be 3.6 sigma. Piper considers it proved that the speed of conduction is the same in all parts of the nerve, in agreement with the previously cited experiments of DuBois Reymond and Englemann on the sciatic nerve of the frog and of Nicolai on the olfactory nerve of the pike.

This review shows, in general, the methods employed in the study of the speed of nerve conduction, their defects and the discordant results obtained by the different experimenters; it shows that nat-

[44] ''Über Ungleichförmigkeiten in der Fortpflanzungsgeschwindigkeit des Nervenprincips nach Untersuchungen am marklosen Riechnerven des Hechtes,'' *Archiv f. Anat. u. Physiol.*, Suppl., 341 ff., 1905.

[45] ''Über die Leitungsgeschwindigkeit in den markhaltigen, menschlichen Nerven,'' *Pflüger's Archiv*, **124**, 591 ff., 1908.

[46] For a detailed description of his method, see Piper, ''Über den willkürlichen Muskeltetanus,'' *Pflüger's Archiv*, **119**, 317 ff., 1907.

ural stimuli should be used on nerves normally situated in the body, and that the great drawback in the determination of the speed of sensory conduction is the lack of knowledge of the cerebral processes involved, where such a large proportion of the time is consumed; it shows also that the early attempts to find the time consumed in the cerebral processes, or the reduced reaction time, by eliminating from the total time, the other factors such as the motor and sensory time, the latent time of the sense organs, etc., are invalid, since these very factors have not been reliably determined.

CHAPTER II

SPEED OF CONDUCTION THROUGH NERVE CENTERS

Numerous attempts have been made to estimate the speed of conduction through nerve centers, especially those in the brain, by subtracting, from the total reaction time, the time spent in peripheral conduction, and by comparing the simple reaction time with the time for a more complicated reaction, *e. g.*, that involving choice or association. Thus the time spent in a choice reaction in addition to that of the simple reaction was assumed to be the time necessary for the particular mental process involved in choice. Only those experiments are of interest here, which attempt to calculate the speed of conduction through some definite center in the brain or cord. However a case worked out by Exner[1] may be mentioned, in which he found the reduced reaction time for a simple psychic process to be 8 sigma. Donders also found the central time for a choice reaction to be from 30 to 140 sigma. The conduction paths involved in such complex psychic processes are unknown and therefore the time lost can not be attributed to a particular center. In this study the conscious factors of a reaction will not be taken into account, for it is no longer considered necessary to analyze reaction times into the perception of the impression, its apperception, and the volitional release of the motor response. The process is probably an acquired cerebral reflex, which may be accompanied or followed by consciousness. The perception and apperception probably take place after the discharge of the motor impulse. The volitional process consists in preparing the motor impulse, which is reflexly discharged.[2]

I. Anatomical Characteristics of Nerve Centers

Anatomically, the brain is made up of the same elements as the peripheral system of nerves, but it is infinitely more complex. It consists of centers or collections of nerve cells and bundles of fibers coursing in various directions and connecting different centers. The bundles of fibers make up the white matter of the brain and are quite similar to the peripheral nerves, the difference being that in

[1] See Schäfer, "Textbook of Physiol.," **2**, 612, 1900.

[2] See Cattell and Dolley, "On Reaction Times and the Velocity of the Nervous Impulse," *Memoirs of the Nat. Acad. of Sciences*, **7**, 394, 1893.

the former the sheets of connective tissue and the primitive sheath are absent. The centers, or the gray matter, consists of nerve cells, their dendrites, terminations of axons and the neuroglia. The last is not considered as nerve tissue and its function is to support and form a framework for the more delicate nerve structure.[3]

Before the introduction of the neurone theory, the cell body was considered the nerve unit and the probable seat of the psychic processes. Gerlach in 1870 formulated the "nerve net" theory, that the dendrites and other cell branches are connected into a dense and continuous mass in the gray matter. But during the next 20 years the use of stains and additional microscopical work laid the foundation for the neurone theory, according to which the cell, with dendrites and axon, forming one neurone, is the unit. The cell, treated with the Nissl stain, showed a multitude of small granules of various shapes, in addition to the nucleus. Certain conditions such as extreme fatigue, poisoning and the like, changed the appearance of these granules or caused them to disappear entirely. The stain used by Bethe and others shows numerous fibrils passing through the cell, and in the case of the vertebrate brain and cord, these fibrils pass from axon to dendrites (of the same cell) without interruption. These facts, along with the evidence from the Golgi stain and from degeneration of the axone after its section from the cell-body, suggest that the cell-body is merely the store-house from which the neurone receives its nourishment and that conduction is independent of it. In certain of the invertebrates the motor neurones are unipolar, the dendrites and axons being continuous, and connected to the cell-body by only a single thread. Bethe,[4] in an experiment on the crab, destroyed the cell-bodies collected at the outside of each ganglion and found that conduction over the neurone was not disturbed, until the nerves degenerated through lack of nourishment. He thus found positive proof that the normal activity of a ganglion can occur without the presence of the cell-body. He is supported in his conclusion by the experiments of Steinach[5] and Langley[6] on the spinal root ganglia, and by those of Exner,[7] and

[3] For a discussion of the neuroglia and references to works on the subject, see Ladd and Woodworth, "Physiol. Psych.," 144 ff., 1911.

[4] "Das Centralnervensystem von Carcinus Maenas," *Archiv f. Mikroscop. Anat.*, L., 629 ff., 1897.

[5] "Über die centripetale Erregungsleitung im Bereiche des Spinalganglions," *Pflüger's Archiv*, **78**, 291–314, 1899.

[6] "On the Stimulation and Paralysis of Nerve Cells and of Nerve Endings," *Journ. of Physiol.*, **27**, 224–236, 1901.

[7] "In welches Weise tritt die negative Schwankung durch das Spinal Ganglion?" *Archiv f. Anat. u. Physiol.*, 567–570, 1877.

Moore and Reynolds,[8] who found no delay in transmission through the spinal root ganglia, a delay which is characteristic of central conduction.

The nerve-cell branches in the centers are continuous with the nerve trunks in the periphery and differ from them only in size. It does not seem likely that conduction over them should be so different from that in nerve trunks as to account for the peculiarities of central conduction.

Therefore the only anatomical structures which exist in a center in addition to those found in a nerve fiber, are the surfaces of separation between neurones. Since no "reflex arc" is composed of less than two neurones, there must be at least one surface of separation in every center. Thus from a histological point of view, the "synapse,"[9] or surface of separation between neurones, becomes the critical point in the study of the cerebral processes.

II. Physiological Characteristics of Nerve Centers

Functionally, the cerebral process is essentially one of conduction, and certain definite differences[10] obtain between conduction in the centers and in the peripheral nerves, while many other differences seem to apply only to special cases. Those most generally recognized are: (1) Slower Speed of Conduction, (2) Irreversibility of Direction of Conduction, (3) Fatiguability of Centers, (4) Resistance to the Passage of a Single Impulse, Lessened by a Succession of Stimuli, (5) Great Variability of the Threshold Value of Stimuli, (6) Refractory Period, (7) Slight Dependence of Effect upon the Strength of the Stimulus, (8) Greater Susceptibility to Changes of Chemical Environment. These differences can be best accounted for by the existence of synapses in the nerve paths. Whatever the nature of the nerve process may be, these surfaces of separation would produce just such changes in conductivity as have been found.

[8] "The Rate of Transmission of Nerve Impulses through the Spinal Ganglia," *Physiol. Centralb.*, **12**, 501, 1898.

[9] This term was first applied to the surfaces of separation by Foster, "Textbook of Physiol.," Pt. 3, 929, 1897.

[10] For a detailed description of these differences between central and peripheral conduction and their relation to the properties of the synapse, see Sherrington, "Integrative Action of the Nervous System," 14 ff., 1906. See also Bethe, "Die Theorie der Centrenfunktion," *Ergebnisse der Physiol.*, **5**, 250 ff., 1905.

III. Theories of the Process Occurring at the Synapse

Two general theories have been advanced in explanation of the process occurring at the synapse, the mechanical theory and the physico-chemical theory. Of the first may be mentioned the contact theory proposed by Duval[11] and Rabl-Ruckhard.[12] This theory supposes a certain motility in the nerve fibrils, which may by retraction interfere with conduction and produce sleep, narcosis and the like, and by protraction, facilitate conduction and cause states of excitement, etc. Grounds for the theory were found in the similarity of nerve cells with their dendrites to amœba with their pseudopods; and in the appearance of neurones stained by the Golgi method. The nerve centers of animals killed while asleep were compared with those of animals killed while awake, and in certain cases, the synapses, or breaks between the neurones, of the former were larger than those of the latter. Closer study, however, showed these differences to be the result of defective staining.

This mechanical theory has been succeeded by the physico-chemical theory of Sherrington. Instead of explaining the peculiarities of centers by motility of the nerve fibrils, he assumes surfaces of separation which must exist between two cells not in actual physical continuity. Sherrington says:[13]

> Even should a membrane visible to the microscope not appear, the mere fact of the non-confluence of the one with the other implies the existence of a surface of separation. Such a surface might restrain diffusion, bank up osmotic pressure, restrict the movement of ions, accumulate electric changes, support a double electric layer, alter in shape and surface tension with changes in difference of potential, alter in difference of potential with changes in surface tension or in shape, or intervene as a membrane between dilute solutions of electrolytes of different concentration or colloidal suspensions with different sign of charge. It would be a mechanism where nervous conduction, especially if predominantly physical in nature, might have grafted upon it characters just such as those differentiating reflex arc conduction from nerve trunk conduction. For instance, change from reversibility of direction of conduction to irreversibility might be referable to the membrane possessing irreciprocal permeability. It would be natural to find in the arc, each time it passed through gray matter, the additive introduction of features of reaction such as characterize a neurone threshold.

Schäfer,[14] in speaking of reaction time, says:

> There is every reason to believe that the additional delay which is characteristic of the passage of nervous impulses through nerve centers, is due to a block

[11] "Hypotheses sur la Physiol. des Centres nerveux," *Compt. rend. de la Soc. de Biologie*, 74, 1895.

[12] "Sind die Ganglionzellen Amoeboid?" *Neurolog. Zentralbl.*, 199, 1890.

[13] "Integrative Action of the Nervous System," 17 ff., 1906.

[14] "Textbook of Physiol.," **2**, 607, 1900.

at each synapse; that in fact the nervous impulses are momentarily arrested at these places of contact of the nerve cells with one another. And it is not improbable that the relative number of these blocks will furnish a key to the differences which are found to obtain in the reaction for different reflexes and psychic processes. The differences of reaction time are too great to be accounted for simply by the fact that the nervous impulses are sent along paths of different length in the various cases; it is more probable that they pass through an increasing number of nerve units, according to the increased complexity of the processes involved. And this, according to the theory of isolated units, would mean the passage across as many synapses, which are the parts of the nerve chain where relative blocks occur.

He considers it quite probable that in the same individual, under the same conditions, the time lost by the block at each synapse is about the same; and further, that, "a careful study of the differences in reaction time and of the lost time in the passage through nerve centers may help to form a conception of the amount of delay which each synapse is responsible for."[15]

IV. Experiments on the Speed of Conduction through Centers

Certain attempts have been made to determine the time consumed in crossing one synapse. Wundt[16] found the time lost in the lower part of the frog's cord, when the stimulation of the posterior root caused a contraction of the gastrocnemius muscle of the same side, to be 8 sigma. For a crossed reflex, that is, the reaction by the leg on the side opposite to where the stimulus is applied, the lost time was 12 sigma. Assuming that the crossed reflex involves one additional synapse, its lost time would be 4 sigma, and if there were two, the time would be only 2 sigma for each synapse. In the same way, the simple reflex, having a lost time of 8 sigma, would involve either two or four synapses. The time lost between the excitation of the cortex of the frog and the response of the gastrocnemius muscle, in addition to the time required for the impulse to travel over the motor nerve, was found by Exner[17] to be 50 sigma. The difference in lost time between the stimulation of the cortex of the frog and stimulation of the medulla, and the muscular response to each, was found by Langendorff and Krawsoff[18] to be 20 sigma. Comparing

[15] *Loc. cit.*, 609.

[16] "Untersuchungen zur Mechanik der Nerven und Nervencentren," 1876, Abth. 2, Stuttgart. See also "Physiol. Psychol.," **1**, 266, 1893.

[17] "Experimental Untersuchungen der einfachsten psychischen Processe," *Pflüger's Archiv*, **8**, 532, 1874.

[18] The difference in time between the stimulation of the cortex and stimulation of the nerve was 36 sigma; the difference in time betwen stimulation of the medulla and stimulation of the nerve was 17 sigma. The difference between

this with the result of Exner, the time lost in the cerebrum proper would be 20 sigma and the time lost in the medulla would be 30 sigma. Other investigators[19] have obtained practically the same results on the frog. The response of the electrical organ of the *Malapterurus* to stimulation of its afferent nerve was studied in the living specimen by Gotch and Burch,[20] who found it to have a lost central time of 8 sigma. This would be the time required for the impulse to pass over the one synapse involved. Wilson[21] found a lost time in the optic lobes of the frog of 15 to 20 sigma. Sherrington[22] calculated the lost time in the flexion reflex of the spinal dog. Assuming the reflex arc to be two thirds of a meter in length, and allowing 5 sigma for the latent time of the muscle, if there were no retardation in the nerve center, the latent time should be 27 sigma. But he found the actual time with weak stimuli to be about twice that amount, or nearly 60 sigma. This would give a lost time in the center of about 30 sigma. But with strong stimuli this time is considerably reduced, so that a total latent time of 30 sigma has been obtained for the flexion reflex. This would give a lost time of about 3 sigma in the center, when the speed of conduction in plain nerve is taken at 30 meters per second. Assuming that there is one synapse in the arc, its time would then be 3 sigma. Sherrington concluded that the stronger the stimulus, the more nearly conduction along a reflex arc resembles conduction over a simple nerve trunk.

Much work has been done on the latent time of the knee jerk, but there is no agreement as to whether it is a true reflex or a direct reaction to mechanical stimulation of the muscles. The latent times found have varied from 25 to 75 sigma in man, which would average about the same as the winking reflex, or 50 sigma. Howell[23] quotes figures for the knee jerk in the dog with its spinal cord severed, 14 to 20 sigma, and in the case of the rabbit of 5 to 8 sigma. These latter figures seem to him too low for reflex time, as just about that much time would be required for conduction over the nerve fiber included in the reflex arc.

According to Wundt,[24] the reflex time increases as the cord de-

the two, about 20 sigma, was taken as the time lost in the cerebrum. "Zur elektrischen Reizung des Froschgehirns," *Archiv f. Anat. u. Physiol.*, 90–93, 1879.

[19] Bubnoff and Haidenhain, *Pflüger's Archiv*, **26**, 1881. Novi and Grandi, "Rivi sper. di freniat.," Reggio-Emilia, 13, 1888.

[20] See Schäfer, "Textbook of Physiol.," **2**, 589, 1900.

[21] "Note on the Time Relations of Stimulation of the Optic Lobes of the Frog," *Journ. of Physiol.*, **11**, 504, 1890.

[22] "Integrative Action of the Nervous System," 19, 1906.

[23] "Textbook of Physiol.," 149 ff., 1908.

[24] *Physiol. Psychol.*, **2**, 314 f., 1893.

velops in complexity. Thus in man a simple reflex with the response on the same side of the cord as the stimulus, requires an average time of 35 sigma. From this he concludes that the processes in the brain consume a much greater time than the spinal reflexes. The results of Exner[25] on the winking reflex are generally quoted as the reflex time for man. He stimulated one eyelid electrically and recorded the reaction of the other eye. The total reaction time was 57.8 to 66.2 sigma. He calculated that the time consumed in sensory and motor conduction and in the latent time of the muscles was 10.7 sigma. Thus the central time would be 47.1 to 55.5 sigma. The time of the winking reflex has been more elaborately determined by Mayhew[26] to be 42.0 sigma. Deducting the peripheral time as given by Exner, the central time would be 31.3 sigma.

Schäfer[27] considers as unsatisfactory, the explanation of the difference between the reaction time of the eye and ear, as being due to the chemical processes in the retina. This process should not be longer than that occurring in the muscles, also a chemical process, and requiring about 2.5 sigma. The remaining difference of about 40 sigma he believes to be due to the greater number of synapses in the nerve chain connecting the retina with the cortex. There are at least two synapses in the retina, while in the cochlea there are none. The first synapse in the auditory path occurs in the medulla oblongata.

To draw a general conclusion from the work done on the question of the time consumed in the passage of an impulse through a synapse, it may be said that such a loss is generally recognized, that the time is highly variable, influenced by the intensity of the stimulus, by practise and by many drugs and stimulants; but under normal circumstances with moderate stimuli, the time may vary in the neighborhood of from 4 to 10 sigma. Schäfer and others consider it quite possible to obtain some idea of this time by careful reaction time experiments applied to appropriate conduction paths.

It is the purpose of the present experiment to compare the reaction time over two conduction paths, identical except that the one path must cross from one side of the brain or cord to the other. Such a crossing would involve one additional neurone, and that is equivalent to saying that one additional synapse, at least, must be traversed. After the elimination of all other disturbing factors, any difference in time may be attributed to the delay in the passage of the impulse through the synapse.

[25] "Über Reflexzeit und Rückenmarksleitung," *Pflüger's Archiv.* **8**, 531, 1874.

[26] "On the Time of Reflex Winking," *Journ. of Exp. Medicine,* **2**, 35–48, 1897.

[27] "Textbook of Physiol.," **2**, 611, 1900.

CHAPTER III

ANALYSIS OF CONDUCTION PATHS

THE extreme complexity of the nervous system, and especially of the nerve centers in the higher animals, makes a study of the time lost in conduction through a synapse seem rather hypothetical. The histological investigation of the human cortex has been only recently undertaken and the results are few and uncertain. Some centers and the association paths connecting them have been definitely traced by the various experimental methods. But the complex structure of a center, or to speak in terms of the neurone, the number of neurones interposed in any center, is yet unknown. Every center contains at least one synapse, and it is quite probable that, in some centers at least, a single synapse becomes a whole system of synapses. But even under these uncertain conditions, it seems worth while to attempt to find a normal time value for the process occurring at the synapses in human beings.

I. CONDUCTION PATHS

The present experiment consists of the reaction, by either the right or the left hand, to visual stimuli upon the center of vision and at various distances toward the periphery of the retina. The conduction paths which are involved in such a reaction probably are:

1. The optic nerves and the optic tracts connecting the retina with the thalamus and the lateral geniculate body; the optic radiations connecting the thalamus and the lateral geniculate body with the visual centers in the occipital lobes.
2. A visual-motor association tract between the occipital lobes and the motor area in the pre-Rolandic region.
3. The descending fibers from the motor area of the cortex, which are included in the direct and crossed pyramidal tracts of the cord; and the motor nerves leading from the cord to the muscles used in finger movements.

The only assumption necessary concerning the centers included in these paths is that their complexity is equal in the two halves of the nervous system, or that there are no differences in the time of conduction through them.

The optic fibers are so distributed in passing through the optic

chiasm,[1] that one half of each retina is represented in each visual area of the cortex, that is, that the right half of each retina sends its fibers to the visual area in the right occipital cortex, and the left half of each retina sends its fibers to the visual area in the left occipital cortex.[2] Thus a lesion in the right occipital cortex causes blindness of the right half of each eye or right-sided hemiopia, and lesion in the left hemisphere causes blindness in the left half of each eye, or left-sided hemiopia. No cases have been recorded in which a lesion of one occipital cortex has affected only one fourth of the retina, or only circumscribed the visual field. Foveal or central vision is not lost in cases of hemiopia, and in the majority of cases of double hemiopia, it is only weakened. Two explanations have been offered for this immunity of the fovea. First, the fibers from the area of clearest vision enter a limited area of the cortex which remains unaffected by the lesions; second, the fibers from the foveal region of each eye are distributed in such a way that each fovea is equally represented in each hemisphere, and that only complete destruction of the two occipital lobes could destroy central vision. The latter view is considered by Monakow as the most probable, in opposition to Munk and Henschen who support the former explanation.[3]

Fig. 1 is a schematic representation of these conduction paths, and is intended to show the connections between the hands and any part of the retina of either eye. In order to make the diagram as simple as possible, intermediate centers such as the thalamus, lateral geniculate body and the medulla oblongata are not shown. Rays of light striking the retina are represented by the lines marked AA', BB', and CC'. All lines which lead to or from the right occipital cortex are broken lines, those leading to or from the left occipital cortex are solid lines. Thus rays B and B' react upon the right occipital cortex and rays A and A' react upon the left occipital cortex. Since the fovea is equally represented in each visual area, two lines are drawn from each fovea, one leading to each visual area. The line joining the two motor areas represents the commissural fibers which traverse the corpus callosum and by which these two regions are associated.

The visual-motor fibers, connecting the frontal and the occipital

[1] For a detailed discussion of the distribution of the optic fibers in the optic chiasm, see Barker, "Nervous System," 785–796, 1901.

[2] For a discussion of the localization of vision in the cortex, see Monakow, "Localisation des Gesichtssinnes im Cortex," *Ergebnisse der Physiol.*, **1**, Pt. 2, 642–645, 1902.

[3] Monakow, *loc. cit.*, 657.

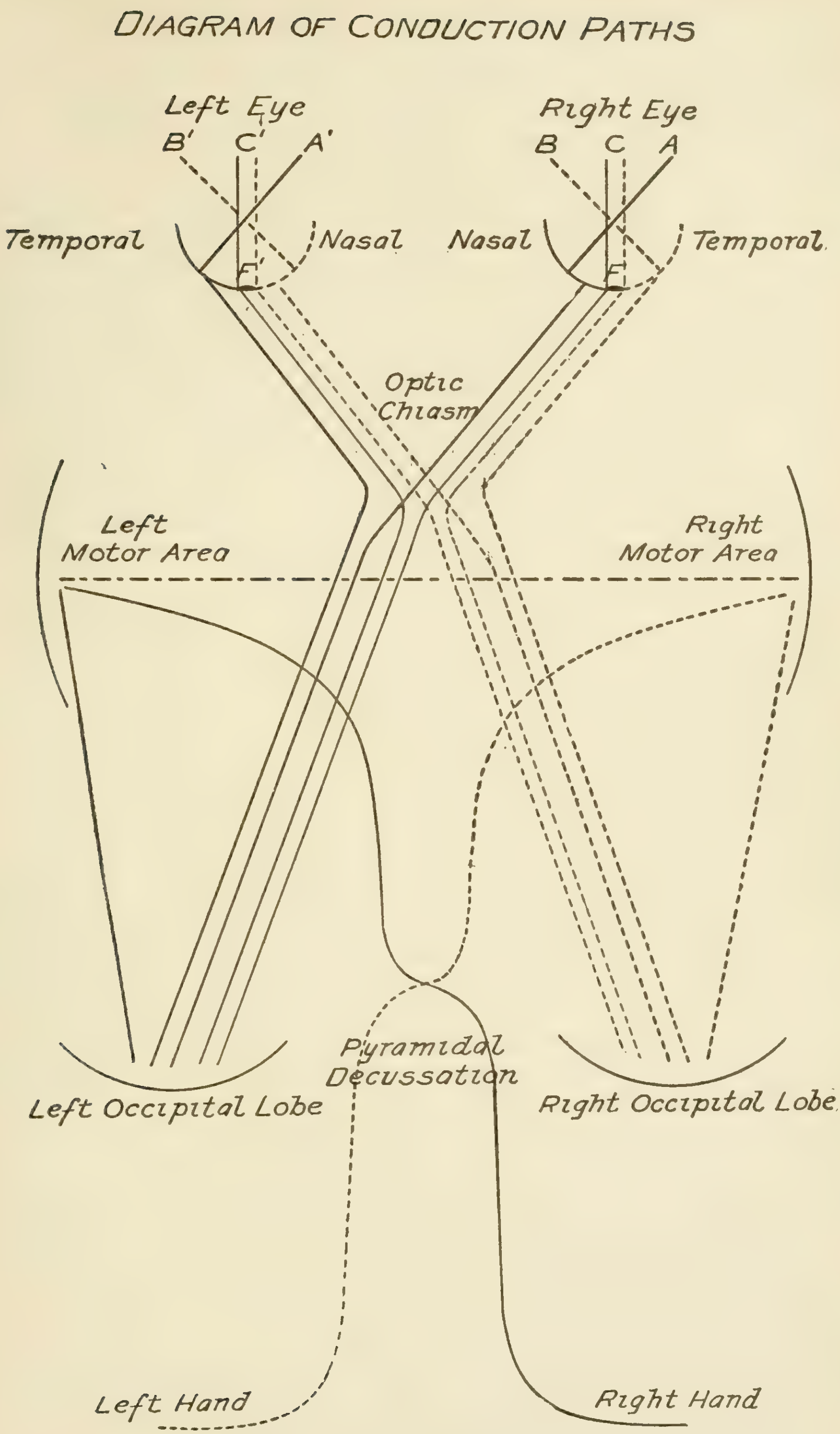

FIG. 1. Schematic Representation of the Conduction Paths.

lobes in the same hemisphere, make up one of the most distinctly marked association tracts in the brain,[4] which is called the superior association bundle.[5] These fibers connect the two regions without passing through any intervening centers.

The fibers from the motor area descend through the internal capsule into the medulla oblongata. There a portion of the fibers decussate and descend into the cord on the opposite side, forming the crossed pyramidal or the crossed cortico-spinal tract. The remaining fibers descend on the same side of the cord and form the direct pyramidal or the direct cortico-spinal tract. All of these latter fibers,[6] however, cross at different lower levels in the cord.[7] From the anterior horn of gray matter in the cord, the motor fibers pass out to the muscles of the hands and fingers.

According to the conduction paths as they have been outlined in the preceding paragraphs, if a light stimulus strikes the retina of the right eye on its temporal side (*B*, Fig. 1), the impulse will be conducted through the optic chiasm to the occipital lobe on the right side. If the hand is to react to this stimulus, the impulse will reach the motor area on the same side, through the visual-motor association tract. From there an impulse generated in the cells of the motor area will be conducted over the descending motor tract to the opposite side of the cord and thence to the left hand. This may be considered as the most direct path over which the impulse can travel.[8] If a stimulus strikes the retina of the right eye on its nasal side (*A*, Fig. 1) the impulse will cross in the chiasm to the left occipital lobe. By way of an association tract to the motor area and the descending motor tracts, the impulses, crossing in the cord, will finally reach the right hand. As may be seen from the diagram, the relations of the retinal areas of the left eye to the hands are reversed, so that the most direct path from the temporal side of the right eye and from the nasal side of the left eye would lead to the left hand, while that from the nasal side of the right eye and the temporal side of the left eye would lead to the right hand.

Since the center of vision of each eye is represented in each visual area of the cortex, a light stimulus striking the fovea of either eye (*C C′*, Fig. 1) would have paths equally direct and open to each hand.

[4] See Ladd and Woodworth, "Physiol. Psych.," 223, 1911.

[5] See Schäfer, "Quain's Anat.," **3**, Pt. 1, 165, 1893.

[6] Certain exceptions to this statement will be made later (p. 29).

[7] Howell, "Textbook of Physiol.," 168 ff., 1908.

[8] With reference to the retina, the relation between eye and hand is crossed, but when the visual field is considered, it is evident that the left hand is most directly associated with the left half of the visual field.

II. Possible Contralateral Connections

If the hand not most directly connected with the part of the retina stimulated is to react, then at some point in its course the impulse must cross to the path leading to that hand. This, in terms of the neurone theory, would mean that one synapse or a system of synapses must be interpolated in order that connections may be established between the two sides of the body. This indirect association between hand and eye might be accounted for in several ways:

1. The impulse might pass over the association path connecting the motor areas in the two hemispheres, a definite tract of fibers occupying the central portion of the corpus callosum.

> Studies of secondary degeneration indicate that through the corpus callosum the activities of a given center in the cerebral cortex of one side are associated with the activities of a precisely similarly located cortical area in the opposite cerebral hemisphere.[9]

Howell,[10] in speaking of the corpus callosum, says:

> So far as the motor regions are concerned, there is some evidence that the connection thus established is between symmetrical parts of the cortex—that is, between parts having similar functions—and we may perhaps regard the corpus as a means by which the functional activities of the two sides of the cerebrum are associated.

Bechterew[11] concludes from a study of the degeneration method, that the corpus callosum connects symmetrical parts of the two hemispheres. (He found, however, that the degeneration may involve convolutions adjacent to the symmetrical parts.) According to Schäfer, if one hemisphere be cut away and electrodes applied to the cut surface of the corpus callosum, the motor centers in the remaining hemisphere may be excited through the fibers of this commissure as readily as if the electrodes were applied directly to the gray matter of the motor area, and the effects produced are similar, though not so easily isolated. That the movements which are produced by excitation of the callosal fibers are brought about through the cortex and not through the fibers which have been described as passing from the cortex of the one side across the corpus callosum directly to the internal capsule of the opposite side, is shown by the fact that they are not obtained if the Rolandic area is destroyed, although the connection between the corpus callosum and internal capsule may not be disturbed. Thus there is direct experimental proof that the same motor impulse may be generated, either directly

[9] Barker, "Nervous System," 1055, 1901.

[10] "Textbook of Physiol.," 216, 1908.

[11] "Leitungsbahnen im Gehirn u. Rückenmark," 558 ff., 1899.

in the motor center, or by impulses coming from the opposite motor center, over an association path.

2. If there were an association tract connecting the occipital lobe of one hemisphere with the motor area of the other hemisphere, an impulse could pass directly to the opposite motor area without involving additional neurones, and with only a very slight increase in the length of path traversed. However, evidence of the existence of such a path is wanting. Wundt[12] says that to some extent the corpus callosum connects cortical regions asymmetrically situated, but he does not state any definite tracts. Schäfer[13] mentions an association tract connecting the occipital lobe of one hemisphere with the temporal lobe of the other. He also says that the Rolandic area receives fibers from numerous other regions than the occipital, but he does not mention any connection between the visual area of one side and the motor area of the other, through the corpus callosum.[14]

3. Some evidence has been adduced, especially by Liepmann and Pick,[15] for the existence of a special coordinating center located in one of the hemispheres, probably in the frontal region, analogous to the speech center, and controlling the more complicated and skilled movements. For instance, in some localized diseases of the brain, there is a loss of the ability to perform very familiar and apparently simple acts, such as lighting a candle or picking a flower. In right-handed persons this center would probably be located in the left hemisphere, similar to the speech center. If a single center controlled such movements as are required for a simple reaction experiment, it would probably be directly connected with each motor area, and impulses could be transmitted from the coordinating center to either motor area over similar nerve paths. But coordinated movements such as are required in the present reaction can be obtained directly by stimulation of the motor area or the cut surface of the corpus callosum. Thus Ladd and Woodworth[16] say:

> The movements obtained by stimulating the motor area are coordinated movements, in much the same way as reflexes are coordinated. That is to say: Neither isolated contractions of single muscles nor general contractions of all

[12] "Physiol. Psych." (trans. by E. B. Titchener), 214, 1904.

[13] "Textbook of Physiol.," **2**, 780, 1900.

[14] In Räuber's "Lehrbuch der Anatomie des Menschen," 185, 1909, he quotes the statement from Cajal, that the callosal fibers do not appear to connect two symmetrical points of the hemispheres; the callosum is rather a complex system, through which the nerve fibers arising in any point of the cortex, influence not only symmetrical cells of the other hemisphere, but through their collaterals, influence many other cells of different convolutions.

[15] See Ladd and Woodworth, "Physiol. Psych.," 254, 1911.

[16] "Physiol. Psych.," 241, 1911.

the muscles in a limb are usually obtained by exciting the motor area. The movements are such as, for example, flexion and extension of the limbs, clenching or opening the fist, pricking up the ear, mastication, turning both eyes to the side, etc. In higher apes, movements of separate fingers can be obtained; but even these are to be regarded as coordinated movements.

In a subject trained in reaction time work, the movements of the reacting finger become virtually reflex. Thus experiment seems to demonstrate that such movements as are required for the reaction of a finger to a stimulus may be obtained without the control of a coordinating center.

4. There is some evidence,[17] both anatomical and physiological, that the motor area of one hemisphere, though principally connected with the opposite side of the cord, is connected to a much slighter degree with its own side. And the presence of such poorly developed paths has been suggested to explain the puzzling cases of return of function after injury to the motor area. Howell,[18] in attempting to explain bilateral control of certain muscles and organs, such as the muscles of the chest and diaphragm, says:

> The motor area on each side may send down a double set of pyramidal fibers, one of which crosses and the other remains on the same side, or the fibers may bifurcate. . . . Some evidence in favor of this view is found in the undoubted histological fact brought out by Mellus and others, that small unilateral lesions in the motor area—the center of the great toe in the monkey, for instance—are followed by degeneration in the lateral pyramidal tracts on both sides, showing that some portions of the motor area send fibers to both sides of the body.

Bechterew[19] mentions the fact that lesions of the motor zone are not followed by degeneration of the crossed and direct pyramidal tracts alone, but also by a slight degeneration in the lateral pyramid on the same side. He opposes the view of Charcot, that the fibers cross back to their original side in the ventral commissure of the cord, and also that of Marchi, that there is a partial crossing of the pyramidal fibers in the region of the corpus callosum. Bechterew cites the more recent work of Dejerine and Thomas and concludes that there are some uncrossed fibers in the lateral pyramids. However the condition of hemiplegia, or the paralysis of voluntary muscles on one side of the body, as a result of injury to the opposite hemisphere of the brain, has long been known, and gives sufficient evidence of the crossed control of the muscles producing the movements of the limbs.

[17] See Ladd and Woodworth, "Physiol. Psych.," 242, 1911.

[18] "Textbook of Physiol.," 188, 1908.

[19] "Leitungsbahnen im Gehirn u. Rückenmark," 496 ff., 1899.

III. Advantages of Retinal Stimulation for Experiment

Experimental evidence, therefore, seems to point to the association path between the two motor areas, as the path over which an impulse from a given part of the eye must travel in order to innervate the muscles of the hand not directly associated with this part. The possible existence of connecting neurones in the lower centers, which might also perform the function attributed to the corpus callosum, can not be denied, although experiment offers no affirmative evidence. Associations between other parts of the body and the hands probably take place through the corpus callosum, with a resulting difference in the character of the conduction paths similar to the ones described. But the plan chosen in the present experiment seems to be particularly free from the difficulties brought out in the historical sketch of the study of nerve conduction by the reaction time method. These are the differences in reaction time, due to:

1. *Differences in the Sensitivity of the Parts Stimulated.*—As the history showed, this has always been a disturbing factor where either electrical or tactual stimuli are used. But the difference in the sensitivity of the two eyes and of the nasal and temporal sides of each eye, as far as it affects the speed of reaction, may be determined and eliminated by averaging in the final result the reaction for the temporal side of one eye with that of the nasal side of the other eye and *vice versa*. The discussion of this difference will make up Chapter V., and the method of eliminating it will be considered in Chapter VII.

2. *Different Distances of the Parts Stimulated from the Brain.*—When stimulation of different parts of the body, *e. g.*, hand and upper arm, is used in determining the speed of conduction in nerves or nerve centers, the distance of the parts from the brain is a factor to be taken into account. The use of the eyes, and especially of different sides of the same eye, makes all points of stimulation equidistant from the brain center.

3. *Differences in the Speed of Reaction of the Two Hands as a Result of Training or Use.*—Any difference in the time of reaction of the right and left hands, may be found by comparing the reaction time to stimulation of both eyes, in which case the paths to each hand are equal; and it may be eliminated by averaging the reactions of one hand to stimulation of one position of the eye with that of the other hand upon the same position of the other eye. The difference in the reaction times of the two hands will be discussed in Chapter VI., and the elimination of this difference will be considered in Chapter VII.

We may conclude from this discussion that the conduction paths as they are schematically shown in Fig. 1, including the commissural fibers between the two motor centers, are the means of association between the hands and the different retinal areas. We may conclude further that these paths supply the conditions called for in Chapter II., that there shall be two conduction paths identical except that the one path must cross from one side of the brain or cord to the other and that the difference in the reaction time over these two paths will give the time consumed in traversing the synapse or synapses involved.

CHAPTER IV

THE EXPERIMENT

I. The Apparatus

The conditions of the experiment require that the subject and the experimenter shall be in the same room. Although this arrangement has the advantage when frequent changes in the position of the subject necessitate the presence of the experimenter, as in the present case, it has a disadvantage in that the contact devices of the apparatus must be noiseless. This qualification excludes the pendulum contact apparatus and the ordinary exposure screen methods. The apparatus adopted for this experiment, and shown schematically in Fig. 2, is a modification of that used by Froeberg, and described in his study on "The Relation between the Magnitude of Stimulus and the Time of Reaction."[1] It consists of a heavy iron wheel 92 cm. in diameter, with a wide rim, and with its axis in a vertical position. An electric motor drives the wheel by means of a belt around its circumference. The belt drive gives greater satisfaction than the worm gear previously used, because it is less complicated and practically noiseless. The motor is supplied with current from the University light circuit, through rheostat[1] (see diagram). If there are any irregularities in the current, they are counteracted by the momentum of the heavy wheel, which preserves a uniform speed of one revolution in 1.25 seconds.

The light stimulus, a two-candle-power electric lamp, mounted in a small metal box, with an aperture one centimeter in diameter, is fastened to the rim of the wheel. The metal box extends 4 centimeters beyond the circumference of the wheel, and thus makes the diameter of the path traversed by the light stimulus 100 cm. Traveling at the rate of one revolution in 1.25 seconds, the light would have an exposure through a one centimeter opening, of about 4 sigma. The current for the light is obtained from the university circuit, and passes through rheostat[2]. The fluctuations in this current are too slight to produce a noticeable change in the brightness of the stimulus. The artificial light as a stimulus is less convenient than daylight reflected from white paper, but we chose the former so that a

[1] Archives of Psychology, No. 8, 1907.

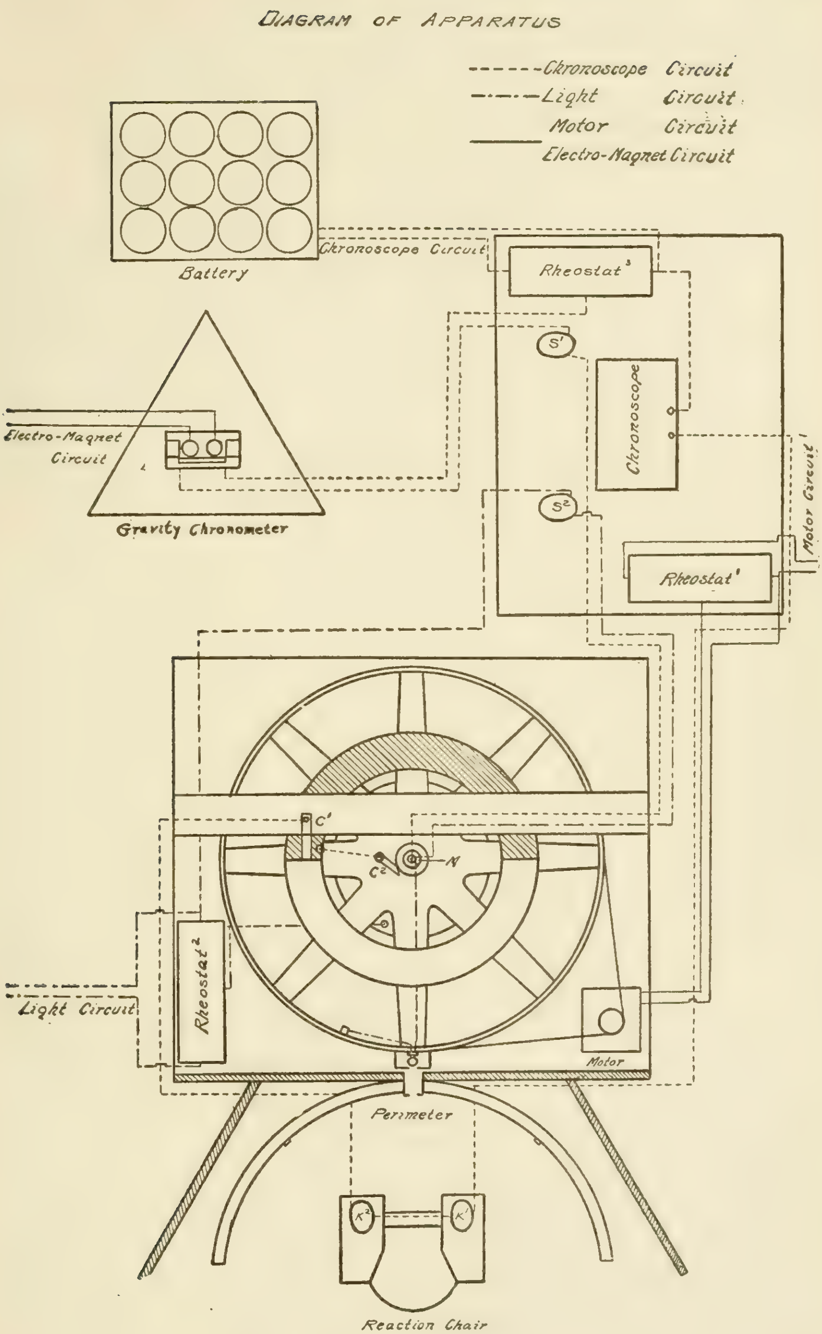

FIG. 2. Diagram of the Apparatus.

comparative set of experiments could be conducted at night with the same stimulus. The light passes through a double screen of ground glass and pale blue glass. The latter counteracts the excessive yellow rays of the carbon filament, so that examination with the spectroscope showed the result to be a pure white light. We took this precaution so that the reactions might serve as a standard with which to compare reactions to colored stimuli of the same size. The stimulus was made strong enough to reduce the mean variation of the reactions as much as possible, without producing troublesome after-images. The light current, after passing through rheostat,[2] goes through a switch S^2, shown at the right of the chronoscope in the diagram, then by means of the mercury cup *M,* fastened to the stationary shaft of the wheel, the current is carried to the lamp on the rim of the wheel. From the lamp the circuit is completed through the wheel, and from its base, the current returns to the rheostat.[2]

To record the reaction times, we were fortunate in being able to use the Hipp chronoscope modified by Cattell and Dolley.[2] The arrangement of the chronoscope circuit is a modification of that used by Froeberg. The upper side of the wheel carries a circular track, half of which is brass and half a non-conducting fiber. A copper brush C^1, fastened to a bridge above the wheel, rests upon the moving track and closes the chronoscope circuit, when it touches the brass half of the track. As the brush passed from brass to fiber, the quality of the sound changed so much that there was danger of a false reaction to the sound. But a slight change in the form of the brush contact made it practically noiseless.

The lamp is adjusted on the rim of the wheel so that the instant it is exposed to the subject, the chronoscope circuit through the brush C^1 is closed. The lamp occupies this position in the diagram. The current for the chronoscope is supplied by 12 gravity cells. We first tried the university light circuit, and later a 6-volt current from a motor dynamo receiving its power from the university circuit. But in both these cases the mean variation of the chronoscope readings was too high, showing a fluctuation in the current. The chronoscope circuit contains the Cattell gravity chronometer,[3] which is set

[2] The description of the changes made in this chronoscope is contained in ''On Reaction Times and the Velocity of the Nervous Impulse,'' *Memoirs of the Nat. Acad. Sci.,* 7, 1893. The principal one of the changes consisted in rewinding the electro-magnets with coarser wire, which reduced the time of magnetization and demagnetization.

[3] This chronometer is described in detail and illustrated in *Memoirs of the Nat. Acad. of Sciences,* 7, 1893.

to give a standard time of 200 sigma, since the reactions to the light vary about that time. The electro-magnet of the chronometer receives its current from an independent circuit, as shown in the diagram. Ten chronometric readings were taken at the beginning of each experiment and the current so adjusted that the mean variation of the readings per 100 sigma should be less than one sigma. After the current passes through rheostat[3], it traverses the chronometer circuit, then the switch S^1 at the left of the chronoscope. From this switch it is carried to the stationary shaft of the wheel, where it passes down through the center of the mercury cup M, and insulated from it, into a collar upon the shaft. The brush C^2, upon the moving wheel, carries the current from the shaft to the semicircular brass track. By means of the brush C^1, the circuit is completed through the track, then passes through the two reaction keys, K^2 and K^1, and finally through the chronoscope into rheostat[3].

The light stimulus appears to the subject at the middle point of a perimeter 92 cm. in diameter. Black screens are arranged above, behind and at the sides of the perimeter, so that the subject sees none of the mechanism and when in position, always faces a black background. There is a diaphragm at the aperture in the perimeter, a distance of 12 cm. in front of the light. While the stimulus is being observed with both eyes, the diaphragm is thrown wide open. When only one eye is being stimulated, it is adjusted so that the light can be just completely seen through it. The value of this arrangement is that the subject can see the light as a complete circle only when the eye is in its proper position, and any variation from this position can be detected at once. A guide marks the center of the circle, of which the perimeter is an arc. It consists of a hinged metal strip suspended from above, with a one cm. hole in its lower end. This marks the proper position for the subject's eye, and is raised out of the way during the test. However after some practise the subject can assume the correct position with the aid of the diaphragm alone, and in that case the guide serves only to maintain the proper distance of the eye from the stimulus. The perimeter is marked off in degrees and carries two white movable fixation points, one centimeter in diameter, one on each side of the diaphragm. The total distance of the eye from the stimulus is 58 cm. According to Howell,[4] the average diameter of the fovea is usually given as lying between .3 and .4 mm. Lines drawn from the ends of its diameter

[4] "Textbook of Physiol.," 322, 1908.

to the nodal point[5] of the eye would subtend an angle of one degree, and therefore an object in the external world whose visual angle would fall within one degree would be imaged on the fovea. An object subtending an arc of one degree at a distance of 58 cm. must be 1.01 cm. in diameter. According to these measurements then, an image of the present stimulus which is one centimeter in diameter, must fall within the limits of the fovea.

The reaction chair consists of an ordinary swivel chair, adjustable in height. On each of its two broad arms, is a reaction key of the Morse type. Between these arms a sliding frame supports the head rest (not shown in the diagram), which is put in place after the subject is seated, and is firmly fastened to the chair by means of a thumb screw. The head rest gives a firm support for the forehead and chin, the height of the chin rest being adjustable. Additional supports for the head proved a disturbance rather than an aid in preserving a fixed position. Lateral supports upon the temples and a cast for supporting the jaws were tried and discarded. The work which has been done on visual fixation shows that, even when the head is firmly fixed, eye movements still occur. McAllister[6] found that the eye, when fixating, unconsciously wanders over an area somewhat less than one degree. Dodge[7] distinguishes three classes of head movements, a pulse movement occurring somewhat rhythmically, movements corresponding roughly to the breathing, and other miscellaneous, irregular movements. But when the head is pressed against a support, the movements occurring in five minutes are not greater than about one half of a degree. Such slight variations in the position, both of the eyes and of the head, can be disregarded in the present experiment, where the fixation required at one time is never longer than a few seconds. The subjects seemed to experience no difficulty in holding their fixation, as far as they could judge from introspection. The fixation points are white upon a black background and the aperture through which the stimulus appears also shows black, so that the only bright spots in the visual field are the fixation points. Dodge[8] studied the reaction time of

[5] The nodal point of the eye lies in the crystalline lens, 7.3 mm. from the anterior surface of the cornea. It is the center of an ideal refracting surface, equivalent to the sum of all the refracting mediums of the human eye. See Howell, *loc. cit.*, 289 ff.

[6] "The Fixation of Points in the Visual Field," *Psych. Rev.*, Mon. Suppl., **7**, 17–53, 1905.

[7] "Eine experimentelle Studie der visuellen Fixation," *Zeitschr. f. Psych. u. Physiol. des Sinnesorgane*, **52**, 321–422, 1909. "Study of Visual Fixation," *Psych. Rev.*, Mon. Suppl., **8**, 1907.

[8] "Study of Visual Fixation," *Psych. Rev.*, Mon. Suppl., **8**, 13 ff., 1907.

eye movements to peripheral stimulation and found an average time of about 165 sigma, that is, when a stimulus strikes the periphery of the retina, the movement to bring the center of vision to that point requires 165[9] sigma. The duration of the stimulus in the present experiment is only about 4 sigma, and the long reaction time of the eye would preclude any change of fixation between the appearance of the stimulus and its disappearance.

II. Methods

The experiments were conducted during the day between the hours of nine and four. Although experimentation with visual stimuli in daylight has certain disadvantages, it is free from the complication of dark adaptation and can be conducted under more nearly natural conditions. The apparatus is situated in a large room, midway between two windows, in such a way that the only light falling within the hood covering the perimeter, is that reflected from the white wall behind the subject. At the suggestion of Professor Cattell, the intensity of the light in the room is regulated as follows: Under the hood and directly above the middle point of the perimeter, a small photometer was fastened. This consists of a white screen, an adjustable rod and a two-candle-power electric lamp (not shown in the diagram). At the beginning of the experimental work, a day of average brightness was selected and the adjustable rod so placed that its shadow cast on the screen by the lamp would be just perceptible. During all the following tests, the light in the room was regulated by two sets of curtains, so that this shadow would again be just perceptible. No experiments were made on cloudy days, which were too dark to allow such adjustment. Although this device is not extremely accurate, it is considerably more so than ordinary observation. The work of Froeberg[10] shows that a change of 50 per cent. in the light will make a difference of only a few sigma in the reaction time, so that all variations in the reaction time due to great changes in the light intensity have been eliminated.

Three methods of applying the stimulus to different parts of the retina suggest themselves. Probably the best method would be to provide a permanent fixation point for the eye in the center of the perimeter and to move the stimulus to the different eccentric posi-

[9] This time was found by a method devised by Erdmann and Dodge in 1899. Later results obtained by the photographic method, although not the same, confirm the belief that the reaction time of the eye is relatively long.

[10] "The Relation between the Magnitude of Stimulus and the Time of Reaction," Archives of Psychology, No. 8, 1907.

tions. But the nature of the apparatus controlling the stimulus made this method impossible in the present case. The method adopted at first and used for the stimulation of points 45 degrees from the fovea, consisted in keeping the head fixed in such a position that when the eyes looked straight to the front, the stimulus should strike the fovea. Then when an eccentric position was to be stimulated, the head remained in its original position, and the eye was turned in its socket 45 degrees in that direction. When one attempts to hold this position for a time, the strain on the muscles of the eye can be distinctly felt. But during the experiment the subject looked straight ahead until the ''ready'' signal warned him to assume the peripheral position. Thus each fixation lasted only two or three seconds. A certain amount of attention had to be directed to the fixation process by this method, a factor which it seemed best to eliminate. The third and most satisfactory method was then adopted, that is, the use of the revolving reaction chair described on page 36. The stimulus appears at the middle point of the perimeter and the whole body is turned, so that the eye can fixate any point and still keep a normal position in its socket. All of the subjects considered reaction to peripheral stimuli much easier with this arrangement.

During the study of the reaction to peripheral stimuli, one eye was stimulated at a time. The other was closed by a black screen so fitted that it would allow the eye to remain open and move about freely, and yet not cut off the rays of light directed toward the temporal side of the other eye. No form of screen proved perfectly satisfactory, and since the smallest ray of light entering the covered eye disturbed the subject, it was necessary in some cases to apply a strip of adhesive plaster on the nasal side across the nose. It was this difficulty which prevented our testing points more peripheral than 45 degrees. In every case, after a short time the subject became so adapted to the screen that he was unable to say which eye was covered and which was being stimulated.

At the beginning of each experiment, the chronoscope was tested and the light conditions regulated. The subject seated himself in the reaction chair and the head rest was fastened in position. Two warning signals, ''Get Ready'' and ''Now,'' were given about a second apart. Two signals were used rather than one mainly because subject *T,* trained in reaction work, was accustomed to this method, and only one proved confusing to him. Between the ''Now'' and the application of the stimulus, the interval ranged from one to two seconds. The chronoscope was started and when running properly, the ''Ready'' signal was given, which was the sign for the

subject to press the reaction key, preparatory to its release. Immediately after the lamp had passed the aperture, the experimenter turned on the light by closing switch S^2. This gave nearly one revolution of the wheel for the light to reach its maximum brightness. About the same time, the "Now" signal was given. The switch S^1 in the chronoscope circuit is closed after the brass track has passed beyond the brush contact C^1. At the instant the stimulus is given the circuit through the brush and track is closed and the chronoscope begins to record, and continues until it is stopped by the release of the reaction key.

One *series* consists of 10 reactions in each of 12 positions, that is, upon the fovea, and at a given distance, *e. g.*, 45 degrees, upon the temporal and nasal side of each eye with each hand, making 120 reactions in all.

Right Eye	Right Hand,	Fovea,	Temporal,	Nasal.
	Left Hand,	Fovea,	Temporal,	Nasal.
Left Eye	Right Hand,	Fovea,	Temporal,	Nasal.
	Left Hand,	Fovea,	Temporal,	Nasal.

This series required an average time of 40 minutes, including several minutes to change the eye screen, and to allow the eye to become adapted to the light; also a short time required to change positions on the same eye. A *set* of reactions consists of ten such series,[11] or 100 reactions in each position, making 1,200 reactions in a complete set. The various positions in each series were so arranged that two series never began or ended with the same position. This precaution would, in a whole set of experiments, tend to equalize any practise and fatigue effects, which might appear in a single series. In cases where more than one series was taken in a day, an hour elapsed between them.[12] Whenever a subject was not in good physical or mental condition, or as soon as he felt fatigued, work was discontinued.

A whole set of experiments at a given peripheral distance, *e. g.*, 45 degrees, was completed before another at a different angle was begun. Such an arrangement might introduce a pronounced practise effect, but the method of treating the results, as will be explained later, makes a direct comparison of the different sets and even of the different series unnecessary.

Four persons have acted as subjects, *T*, *P*, *A*, and *M*, all of them

[11] Cases in which a set was made up of only five series will be noted later.

[12] An exception to this statement in the case of subject *A* will be mentioned later.

graduate students in the department of psychology. The first two were well trained in reaction time work, and the last two had served as subjects at various times. An intensive study of a few subjects was undertaken rather than a few tests of a large number of persons, on account of the difficulty in getting trained subjects who could spend the large amount of time required for such an experiment.

CHAPTER V

THE REACTION TIME TO STIMULATION OF DIFFERENT RETINAL AREAS

Before the speed of nerve conduction can be calculated by the reaction time method, whether conduction be over a nerve trunk or through a nerve center, certain facts regarding the parts stimulated and the parts reacting must be known. In the historical sketch given in Chapters I. and II., the difficulties and errors from these sources were pointed out. At the close of Chapter III., in which the conduction paths to be studied in this investigation were outlined, three sources of error were indicated. Two of these sources of error are involved in comparing the reaction time by the hands to stimulation of the temporal and nasal sides of the retina; first, the quality of the different retinal areas showing itself in the difference of reaction time; and second, the difference in the reaction time of the right and left hands. The first of these problems will be discussed in this chapter, and the second in Chapter VI.

I. Historical

In 1879 Hall and Kries[1] reported an experiment on "The Dependence of Reaction Time upon the Point of Stimulation." They compared the reaction times to stimulation of different points on the skin, but concluded that it is impossible to make the stimuli on different parts of the body comparable. They then measured the reaction times to visual stimuli seen by direct and indirect vision. A Geissler's tube furnished the stimulus. The head was held in a fixed position by a support, so arranged that the eye could be directed to different points in the visual field. One eye was used at a time, the other being held shut. The question of the intensity of the stimulus on the different parts of the retina was disregarded by the investigators, since the results were so definite that a large variation in the intensity of the stimulus made little difference in the results. The fovea and points 30 and 60 degrees on each side and above and below the fovea were tested on two subjects. The average reaction times for each position are as follows:

[1] "Über die Abhängigkeit der Reactionszeit vom Ort des Reizes," *Archiv f. Anat. u. Physiol.*, Suppl. 1–10, 1879.

Subject	Fov.	Temp. 30°	Nas. 30°	Above 30°	Below 30°
H	221	261	245	248	287
K	171	196	186	186	215
		Temp. 60°	Nas. 60°	Above 60°	Below 60°
H	——	281	263	263	291
K	——	277	216	184	248

The number of reactions from which the averages are calculated and the mean variations are not given. No statement is made as to whether or not the value for each position is an average of the two eyes for that position. Hall and Kries concluded from their experiments that the time for the temporal[2] side of the retina is always greater than for the nasal side, and for the lower part greater than for the upper part of the retina. They considered the differences due to the differences in the central, or reduced reaction time, on the assumption that those parts which are most used or exercised have the quicker central time. The central time does not seem to depend on the sensitivity of the part stimulated, according to them, since the reaction time upon stimulation of the tongue is longer than in the case of stimulation of the forehead, although the tongue is said to be twenty times as sensitive as the forehead.

The work of Kästner and Wirth,[3] on the stimulation of different retinal areas, is not directly comparable with the present work or with that of Hall and Kries. In their case certain areas of the retina were associated with one hand or the other, so that when the light stimulus fell upon a certain area the proper hand must be chosen to react. Moreover the points stimulated were not the same in the case of these experimenters. But by taking the averages of certain of their positions at the highest stage of practise, Kästner and Wirth found some similarity between their results and those of Hall and Kries, in that the direction of the differences was the same. In both cases the nasal side and the upper side of the retina gave faster times than the temporal and the lower side. These investigators agree with Hall and Kries that the differences are due rather to central than to peripheral causes.

II. Results of the Present Study

Although the previous experiments upon the reaction time of the hands to stimulation of the periphery of the retina have been by no

[2] The positions as they are given originally by Hall and Kries refer to the visual field and not to the retina. The figures have been reversed in order that they may correspond to the system used in the present work, in which the positions refer to the retina.

[3] ''Die Bestimmung der Aufmerksamkeitsverteilung innerhalb des Sehfeldes mit Hilfe von Reactionsversuche,'' *Psychol. Stud.*, **3**, 361–392, 1907; **4**, 139–200, 1908.

means exhaustive, they indicate that certain differences do exist. Hall and Kries considered the differences between the fovea and the temporal and nasal sides of the retina to be so pronounced, that certain disturbing elements were disregarded by them. Kästner and Wirth confirmed these results.

This study, having a purpose beyond the determination of the reaction time to stimulation of the peripheral retina, has not covered so wide a range as that of Hall and Kries, but consists of a more intensive investigation of the reaction time to stimulation of the fovea, and of points 3, 10, 30 and 45 degrees from the fovea in a horizontal plane.

The following tables, I., II., III. and IV., give the general results of these experiments. Table I. gives the averages for the fovea and the temporal and nasal sides of the retina at 45 degrees from the fovea, Table II. gives the same for 30 degrees, Table III. for 10 degrees and Table IV. for 3 degrees.

Each table is made up of 10 series of 10 reactions each for every subject, so that each figure in the tables is an average of 100 reactions.[4] All experiments at the distance of 45 degrees from the fovea were made with the head in a fixed position and the points were fixated by rotating the eye in its socket, as described on page 38. In the case of subject *T*, every series at 45 degrees consisted of 13 reactions upon each position. An average of the 13 reactions was taken and the three reactions farthest from the average were discarded, and a new average obtained from the ten remaining reactions. This correction made so little difference, that the method was discontinued. However 13 reactions were still taken on each position at 45 degrees for the different subjects, so that the figures in Table I. for all but subject *T*, are each the average of 130 reactions.

The experiments at 30, 10 and 3 degrees whose results are given in Tables II., III. and IV., were made with the revolving reaction chair, described in Chapter IV., by which the whole body was turned to an eccentric position instead of the eyes alone. Subject *A* appears only in Table I. as she left the university before the work was completed.[5]

[4] Certain exceptions to this statement will be noted.

[5] The whole set of experiments consisting of 10 series of reactions were made on subject *A* on one day, in two periods of five series each, with an interval of about three hours between them. The intervening period, however, was taken up by another reaction time experiment, so that the work was practically continuous for about eight hours. (An interval of one half hour was allowed for lunch.) The whole day's work was designed as a fatigue experiment in reaction time, but since no fatigue effect appeared in various mental tests given at the

REACTION TIME TO STIMULATION OF DIFFERENT RETINAL AREAS

TABLE I

LEFT EYE, 45 DEGREES

Subject		Right Hand Fov.	Right Hand Temp. 45	Right Hand Nas. 45	Left Hand Fov.	Left Hand Temp. 45	Left Hand Nas. 45
T	Av.	203.5	227.7	221.6	212.3	236.0	226.1
	M.V. ...	*9.6*	*11.2*	*7.6*	*14.2*	*14.3*	*12.1*
A	Av.	193.6	217.3	207.9	190.8	203.4	198.6
	M.V. ...	*9.0*	*14.4*	*11.5*	*9.7*	*9.8*	*9.1*
P	Av.	176.9	204.6	195.7	165.2	193.9	183.8
	M.V. ...	*3.1*	*5.7*	*5.6*	*4.9*	*8.7*	*8.9*

TABLE II

LEFT EYE, 30 DEGREES

Subject		Right Hand Fov.	Right Hand Temp. 30	Right Hand Nas. 30	Left Hand Fov.	Left Hand Temp. 30	Left Hand Nas. 30
T	Av.	196.4	211.2	209.7	191.2	208.8	199.1
	M.V. ...	*6.6*	*9.7*	*7.0*	*6.3*	*8.7*	*7.7*
P	Av.	179.7	188.8	190.7	178.1	192.0	181.9
	M.V. ...	*2.5*	*5.0*	*4.5*	*7.3*	*3.5*	*4.7*
M	Av.	177.8	195.6	193.3	175.9	187.0	182.6
	M.V. ...	*11.6*	*13.4*	*7.6*	*10.2*	*9.1*	*8.8*

TABLE III

LEFT EYE, 10 DEGREES

Subject		Right Hand Fov.	Right Hand Temp. 10	Right Hand Nas. 10	Left Hand Fov.	Left Hand Temp. 10	Left Hand Nas. 10
T	Av.	185.5	190.7	193.5	191.6	203.2	191.2
	M.V. ...	*8.7*	*4.6*	*7.4*	*7.0*	*7.7*	*6.4*
P	Av.	184.1	191.3	187.7	174.3	191.1	176.9
	M.V. ...	*4.7*	*3.8*	*2.6*	*4.6*	*4.7*	*4.7*

TABLE IV

LEFT EYE, 3 DEGREES

Subject		Right Hand Fov.	Right Hand Temp. 3	Right Hand Nas. 3	Left Hand Fov.	Left Hand Temp. 3	Left Hand Nas. 3
T	Av.	185.6	187.1	189.3	188.6	199.8	190.5
	M.V. ...	*4.9*	*4.9*	*3.6*	*4.7*	*10.3*	*6.4*
P	Av.	181.9	190.6	196.9	174.7	189.7	181.7
	M.V. ...	*3.7*	*4.3*	*3.9*	*3.5*	*4.8*	*4.2*

end of the period, or in a comparative study of the reaction times, the results are considered valid for the present study. The order in which the reactions upon the various positions were taken would tend also to equalize the fatigue effect.

REACTION TIME TO STIMULATION OF DIFFERENT RETINAL AREAS

TABLE I (*continued*)

RIGHT EYE, 45 DEGREES

Subject		Right Hand Fov.	Right Hand Temp. 45	Right Hand Nas. 45	Left Hand Fov.	Left Hand Temp. 45	Left Hand Nas. 45
T	Av.	205.2	228.0	223.0	213.4	237.3	222.5
	M.V. ...	*12.0*	*11.9*	*9.1*	*9.0*	*10.9*	*6.5*
A	Av.	194.1	212.3	207.0	186.2	213.0	206.1
	M.V. ...	*9.6*	*13.3*	*12.2*	*13.4*	*10.2*	*11.4*
P	Av.	174.5	201.5	190.5	168.8	201.1	196.0
	M.V. ...	*6.7*	*10.0*	*6.0*	*5.4*	*7.7*	*4.3*

TABLE II (*continued*)

RIGHT EYE, 30 DEGREES

Subject		Right Hand Fov.	Right Hand Temp. 30	Right Hand Nas. 30	Left Hand Fov.	Left Hand Temp. 30	Left Hand Nas. 30
T	Av.	196.9	209.9	202.0	191.3	201.6	203.4
	M.V. ...	*5.5*	*7.1*	*7.1*	*5.6*	*8.3*	*7.9*
P	Av.	178.9	192.0	181.9	174.6	186.2	191.9
	M.V. ...	*2.8*	*4.1*	*6.1*	*3.2*	*5.5*	*5.5*
M	Av.	179.7	198.8	186.5	176.5	190.1	183.7
	M.V. ...	*7.2*	*1.3*	*3.4*	*5.0*	*2.0*	*6.8*

TABLE III (*continued*)

RIGHT EYE, 10 DEGREES

Subject		Right Hand Fov.	Right Hand Temp. 10	Right Hand Nas. 10	Left Hand Fov.	Left Hand Temp 10	Left Hand Nas. 10
T	Av.	185.3	200.7	193.2	192.5	197.8	200.1
	M.V. ...	*6.2*	*5.5*	*6.1*	*7.4*	*7.4*	*6.0*
P	Av.	178.7	189.5	184.0	173.6	181.2	181.7
	M.V. ...	*3.3*	*7.0*	*4.9*	*3.4*	*4.8*	*5.4*

TABLE IV (*continued*)

RIGHT EYE, 3 DEGREES

Subject		Right Hand Fov.	Right Hand Temp. 3	Right Hand Nas. 3	Left Hand Fov.	Left Hand Temp. 3	Left Hand Nas 3
T	Av.	185.3	192.0	189.5	190.9	185.6	196.5
	M.V. ...	*7.2*	*8.7*	*7.6*	*4.3*	*4.6*	*2.9*
P	Av.	182.5	197.9	192.6	176.9	183.8	190.0
	M.V. ...	*4.3*	*4.5*	*5.4*	*3.7*	*6.3*	*4.6*

In Table II. the figures for subject *M* are the averages of only five series, or 50 reactions on each position. All further tests were made upon the two subjects *T* and *P*. At 3 degrees, the averages for subject *T* are made up from five series or 50 reactions on each position.

III. Methods of Calculating the Mean Variations

The mean variations (M.V.) given in the first four tables, are the mean variations of the average of the series of 10 reactions from the general average of all the series, and not of the individual reactions from the general average. Thus they show the variations of the averages from day to day. Mean variations calculated in this way are somewhat lower than those calculated from the individual reactions. The following figures in Table V. show a comparison of three methods of obtaining the variations, in the case of subject *T* in Table I.:

1. The mean variation of the averages of the 10 series from the general average. This is the method used in the first four tables, and accounts only for the daily variations.

2. The mean variation of the individual reactions from the average of the series was first computed. Then an average of these mean variations was obtained. This method does not take account of the daily variations.

3. The mean variation of all the individual reactions from the general average. This includes both the variations from day to day and those within the series.

TABLE V

Comparison of Three Forms of Mean Variation

Method		Right Hand			Left Hand			
		Fov.	Temp.	Nas.	Fov.	Temp.	Nas.	Av.
(1)	Right Eye	12.0	11.9	9.1	9.0	10.9	6.5	10.7
	Left Eye	9.6	11.2	7.6	14.2	14.3	12.1	
(2)	Right Eye	8.4	11.4	10.2	10.5	9.3	8.5	10.0
	Left Eye	10.2	9.0	12.0	8.7	11.1	11.0	
(3)	Right Eye	14.2	15.3	15.0	13.1	13.6	10.5	15.0
	Left Eye	14.2	20.2	14.1	15.9	17.5	15.7	

The variations within a series are about the same as those among the different series, while the variations of the individual reactions from the general average are about 50 per cent. higher than either. All the three forms of variations would be higher in the case selected than in any of the later experiments, since this was the first test made, and the situation was new.

IV. General Conclusions from Tables I.–IV.

Tables I.–IV. show certain facts common to all the subjects: First, that the length of the reaction times on the eccentric positions is always greater than on the foveal position; second, that the

length of the times increases as the distance from the fovea is increased;[6] third, that the reaction times to temporal stimulation are longer than to nasal stimulation. When the results for one eye and one hand are taken alone, this last result is not so clear, on account of the presence of a factor which will be discussed in Chapter VII., but an average of the two hands for any one position shows the temporal side to give the slower reactions. According to the introspections of all of the subjects, the light grows dimmer as it moves toward the periphery of the retina, and it is always dimmer on the temporal than on the nasal side.

V. Practise Effect in Subjects *T* and *P*

Reaction times usually show no great decrease as a result of practise, aside from the general increase in speed due to a growing familiarity with the situation. But in a long series of tests like the present one, where the reaction times at 45 degrees from the fovea were all taken before the distance of 30 degrees was tested, and this in turn before the distance of 10 degrees, practise effect might account for the general decrease in the reaction times as the positions approach the fovea. It seems well therefore to study the practise effect in subjects *T* and *P*, to determine its influence upon the results given in Tables I., II., III. and IV.

In the case of subject *T* considerable practise effect may be noticed in comparing the foveal times throughout Tables I., II., III. and IV. The tables are arranged in the order in which the experiments were made and the foveal times for the different positions are taken in the same way in each case. It would be difficult to ascertain the number of reactions made between each of the sets, as both

TABLE VI

Practise Effect in Subjects *T* and *P*

Subject *T*	Right Eye		Left Eye	
Data from	Right Hand	Left Hand	Right Hand	Left Hand
Table I.	205.2	213.4	203.5	212.3
Table II.	196.9	191.3	196.4	191.2
Table III.	185.3	192.5	185.5	191.6
Table IV.	185.3	190.9	185.6	188.6
Subject *P*	**Right Eye**		**Left Eye**	
Data from	Right Hand	Left Hand	Right Hand	Left Hand
Table I.	174.5	168.8	176.9	165.2
Table II.	178.9	174.6	179.7	178.1
Table III.	178.7	173.6	184.1	174.3
Table IV.	181.6	177.7	183.5	174.3

[6] The averages for subject *P* at 3 degrees are an exception to this statement.

T and *P* acted as subjects for each other in different reaction experiments. The following comparison in Table VI., however, shows the absolute decrease in reaction time for stimulation of the fovea.

In the case of subject *P* the reverse of practise effect appears. The only explanation of this, obtained from the introspection of the subject, is the changed attitude toward the experiment. The subject was well practised in reacting before this work began, and in the course of the long continued experiment, the reactions became reflex. After the first few tests the conscious attempt to make rapid reactions ceased.

VI. Expression of Data in Terms of Differences

It is evident from the preceding study of the practise effect in subjects *T* and *P*, that the absolute values are influenced by practise effect and the variations from day to day due to uncontrollable causes. In Tables I.–IV., one would compare the various distances from the fovea by taking the difference between the general averages for the given distances. These figures would be subject to the above-mentioned errors. If one took the differences between the fovea and the temporal and nasal sides of the retina for each series, and averaged these, he would eliminate from these averages the daily variations and the effects of practise. This method is comparable to the second method of obtaining the mean variations, described in Section III., of this chapter. The average differences between positions at the distances of 45, 30, 10 and 3 degrees from the fovea could then be legitimately compared.

Tables VII., VIII., IX. and X. give the data of Tables I.–IV. in terms of differences thus computed. They give the differences

DIFFERENCES BETWEEN POSITIONS AT VARIOUS DISTANCES FROM THE FOVEA

TABLE VII

Differences between Positions at 45 Degrees

Subject		Right Eye Temp.-Fov.	Right Eye Nas.-Fov.	Right Eye Temp.-Nas.	Left Eye Temp.-Fov.	Left Eye Nas.-Fov.	Left Eye Temp.-Nas.
T	Av.	23.4	13.5	9.9	23.9	16.0	7.9
	P.E.	*1.84*	*1.78*	*2.19*	*2.19*	*1.75*	*2.16*
P	Av.	29.7	21.7	8.0	28.2	18.7	9.5
	P.E.	*1.71*	*1.58*	*1.63*	*1.37*	*1.45*	*1.26*
A	Av.	22.5	16.4	6.1	18.2	11.1	7.1
	P.E.	*1.43*	*1.58*	*1.15*	*1.39*	*1.24*	*1.24*

TABLE VIII

DIFFERENCES BETWEEN POSITIONS AT 30 DEGREES

Subject		Right Eye Temp.-Fov.	Right Eye Nas.-Fov.	Right Eye Temp.-Nas.	Left Eye Temp.-Fov.	Left Eye Nas.-Fov.	Left Eye Temp.-Nas.
T	Av.	11.7	8.6	3.1	16.2	10.6	5.6
	P.E.	*1.09*	*1.49*	*1.54*	*1.59*	*1.48*	*1.39*
P	Av.	12.4	10.2	2.2	11.5	7.4	4.1
	P.E.	*1.01*	*1.77*	*1.82*	*1.13*	*1.33*	*1.48*
M	Av.	16.3	7.0	9.3	14.5	11.2	3.3
	P.E.	*1.20*	*1.57*	*1.25*	*1.36*	*2.37*	*2.00*

TABLE IX

DIFFERENCES BETWEEN POSITIONS AT 10 DEGREES

Subject		Right Eye Temp.-Fov.	Right Eye Nas.-Fov.	Right Eye Temp.-Nas.	Left Eye Temp.-Fov.	Left Eye Nas.-Fov.	Left Eye Temp.-Nas.
T	Av.	10.4	7.8	2.6	8.4	3.8	4.6
	P.E.	*1.50*	*1.56*	*1.79*	*1.56*	*1.52*	*1.59*
P	Av.	9.2	7.7	1.5	12.0	3.1	8.9
	P.E.	*1.01*	*0.94*	*1.47*	*1.28*	*0.90*	*1.28*

TABLE X

DIFFERENCES BETWEEN POSITIONS AT 3 DEGREES

Subject		Right Eye Temp.-Fov.	Right Eye Nas.-Fov.	Right Eye Temp.-Nas.	Left Eye Temp.-Fov.	Left Eye Nas.-Fov.	Left Eye Temp.-Nas.
T	Av.	0.7	4.9	—4.2	6.4	2.8	3.5
	P.E.	*1.94*	*1.67*	*2.49*	*2.55*	*1.46*	*1.97*
P	Av.	11.2	11.7	—0.5	11.8	11.0	0.8
	P.E.	*1.18*	*0.81*	*1.31*	*1.09*	*1.28*	*1.54*

between the temporal side of the retina and the fovea (Temp.-Fov.), between the nasal side and the fovea (Nas.-Fov.), and between the temporal and nasal sides of the retina (Temp.-Nas.). Thus in Table VII., Temp.-Fov. under subject *T* at 45 degrees and on the right eye means that in this position the reaction time upon stimulation of the temporal side of the retina was 23.4 sigma slower than that upon the fovea. In every case but two (Temp.-Nas., Right Eye, at 3 Degrees for both *T* and *P*), the position mentioned first has the slower time, thus Temp.-Fov., Nas.-Fov., and Temp.-Nas. The times for the right and left hands have been averaged in these tables since they are intended to show the relations between the retinal positions themselves. The figures are obtained as follows: The differences between the averages of the series for the given positions are first taken. The average of these series-dif-

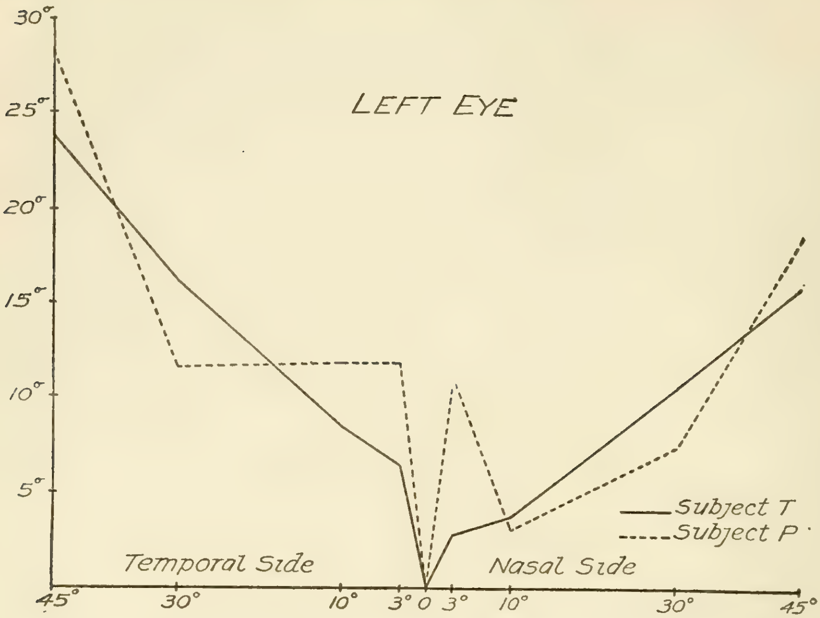

FIG. 3. Curve for the Left Eye, showing the differences in the reaction times for the fovea and the various eccentric positions.

ferences forms the final difference between the two positions. As the reactions by the two hands are combined, each average difference given in the tables is based on 400 reactions. By thus taking the differences between positions within a series as the unit, the variations in the absolute times from day to day through practise and other causes do not influence the results.

The probable error[7] (P.E.) is calculated according to the formula

$$\text{P.E.} = \frac{.8453 \text{ M.V.}}{\sqrt{n}},$$

in which M.V. is obtained according to Method 1, described in Section III. of this chapter, that is, an average of the 10 series-differences is first computed as explained above. Then the variations of these differences from the average are calculated. An average of these 10 variations forms the mean variation. The number of cases, *n*, equals 20.

[7] The probable errors calculated from the differences between series as units are considerably larger than if the individual reactions were taken as units, and the variations of the individual reactions from the average of the series were used, excluding thereby the variations from day to day. In this case *n*, the number of cases, would be 200, as compared with 20 by the method used in this work. To illustrate this fact, calculate the P.E. of the difference between tem-

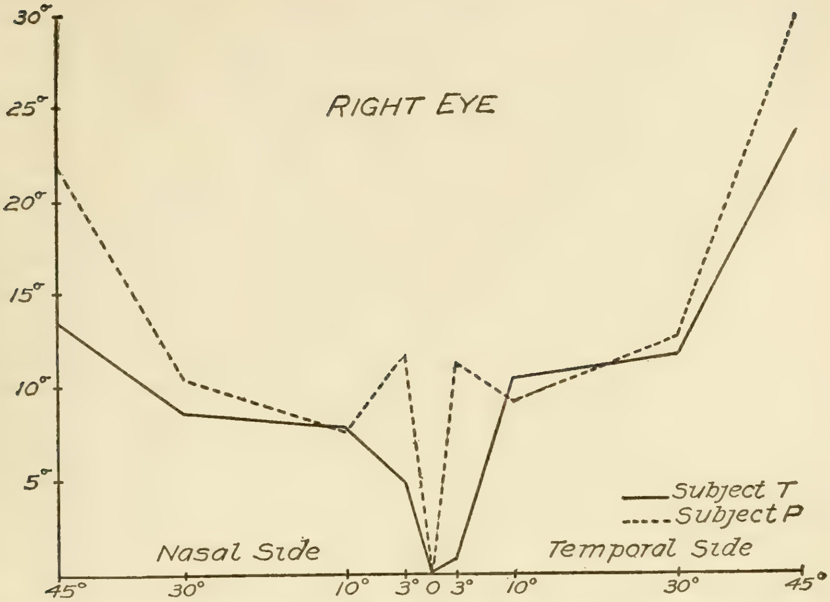

FIG. 4. Curve for the Right Eye, showing the differences in the reaction times for the fovea and the various eccentric positions.

Figs. 3 and 4 show graphically the results given in Tables VII., VIII., IX. and X. Fig. 3 represents the left eye, and Fig. 4 the right eye. In each case the nasal side of the retina is toward the inside of the page and the temporal side toward the outside of the page, that is, they occupy the positions in which one thinks of his own eyes. The abscissas are marked off in degrees and the ordinates are in terms of sigma. Since the data express differences from the fovea, the foveal position is at zero on both the ordinate and the abscissa. Curves for subjects *T* and *P* are given, since they alone completed all the positions. The solid line represents subject *T* and the broken line subject *P*. The curves for the two subjects agree

poral 45 degrees and the fovea, for the right eye of subject *T* (see Table VII.), according to the formula

$$\text{P.E. diff. } A-B=\sqrt{\left(\frac{\text{P.E. } B}{\sqrt{n}}\right)^2+\left(\frac{\text{P.E. } A}{\sqrt{n}}\right)^2},$$

in which P.E. *A* is the probable error for the average of all the reactions upon the temporal position, and P.E. *B* is the same for the fovea. This gives a P.E. of 0.83 sigma as compared with 1.84 sigma, obtained by the method previously described. These lower probable errors are comparable with those obtained by Froeberg in his study on "The Relation between the Magnitude of Stimulus and the time of Reaction," ARCHIVES OF PSYCHOLOGY, No. 8, 1907.

in general, the greatest departure being at three degrees, where *P*'s reaction time is very slow compared with that of *T* and with his own time at 10 degrees. In the curves for both subjects the nasal side is lower than the temporal except at 3 degrees on the right eye in the case of each subject.

VII. Repetition of the Tests at 45 Degrees from the Fovea for Comparative Purposes

At the close of the whole experiment the tests upon *T* and *P* at 45 degrees from the fovea were repeated. It will be recalled from the description of Methods in Chapter IV., that all of the experiments at 45 degrees were made with the subject in a fixed position, that is, the head and body held the same position while the eyes were turned in their sockets to the various eccentric positions. The later work at 30, 10 and 3 degrees was done with the reaction chair, by which the head and body were turned and the eyes kept a normal position in the head. The introspections of the subjects showed that the latter method was more satisfactory. In order to compare these two methods, this second set of experiments was made. The results of both the first and the second methods are arranged in Table XI. for comparison. In the second column, Method 1 signifies the fixed position of the body, and Method 2, the use of the reaction chair. The results for the right and left eye are given separately and the data of the two methods for each subject are placed together. The figures in Table XII. represent differences between the positions named at the heads of the columns.

TABLE XI

Comparison of the Two Methods of Reacting at 45 Degrees from the Fovea

Right Eye

		Right Hand			Left Hand		
Subject		Fov.	Temp.	Nas.	Fov.	Temp.	Nas.
T	Method 1	205.5	228.0	223.0	213.4	237.3	222.5
	Method 2	179.1	207.2	193.5	181.0	205.2	194.3
P	Method 1	174.4	201.5	190.5	168.8	201.1	196.0
	Method 2	180.5	198.2	185.2	178.6	190.8	188.8

Left Eye

T	Method 1	203.5	227.7	221.6	212.3	236.0	226.1
	Method 2	183.9	205.0	193.5	180.8	201.8	190.4
P	Method 1	176.9	204.6	195.7	165.2	193.9	183.8
	Method 2	179.1	205.8	195.1	175.8	201.5	182.5

TABLE XII

Comparison of the Two Methods of Reacting in Terms of Differences

Subject	Method	Right Eye Temp.-Fov.	Right Eye Nas.-Fov.	Right Eye Temp.-Nas.	Left Eye Temp.-Fov.	Left Eye Nas.-Fov.	Left Eye Temp.-Nas.
T	Method 1	23.4	13.5	9.9	23.9	16.0	7.9
T	Method 2	26.2	13.9	12.3	21.0	9.6	11.4
P	Method 1	29.7	21.7	16.1	28.2	18.7	9.5
P	Method 2	15.0	7.5	7.5	28.0	11.4	16.6

Table XI. is interesting since the tests by method 1 were the first taken and those by method 2 were the last, after an interval of over one year. A comparison of the absolute times shows a considerable decrease for subject *T* and an increase for *P* by method 2. This same peculiar difference between the subjects was pointed out (Section III., Chapter V.) in comparing the times for the different distances from the fovea, in which method 2 was used, so that the change is not due in this case to the difference in method. The great change in the amount of the differences shown in Table XII. for subject *P* is due to the increased foveal times, a result probably of some physiological condition of the eye, induced by continual use of central vision in reading. However the results by Method 2 do not change the relative values for the positions at 45 degrees.

VIII. Comparison of the Reaction Times with other Retinal Qualities

A great number of investigations have been made of the qualities of peripheral vision, such as sensitivity, acuity, the extent of the visual field, the appearance of flicker, the apparent size of objects and the speed of movement on different parts of the retina. The results of these investigations will be compared with the present study.

1. Astronomers long ago discovered a difference in the *sensitivity* of the central and the peripheral parts of the retina by the fact that a faint star could be seen best in indirect vision. Numerous investigations have been made upon the light and the dark adapted eye, with varying results. A discussion of the subject may be found in "Über die Helligkeitsempfindung im indirecten Sehen," by Kirschmann.[8] In the same report he gives the results of his own work on retinal sensitivity. He compared the sensitivity of the fovea with the different eccentric positions in the following way: The subject looked directly at a revolving disc of black and white sectors and ad-

[8] *Phil. Stud.*, **5**, 447–497, 1888–1889.

justed a second disc seen by a given peripheral portion of the retina, so that its brightness should be equal to that of the first disc. The actual difference in brightness value of the two discs was then calculated and expressed in terms of the foveal brightness. The following curves, Fig. 5, are constructed from such ratios, given in Tables 5*A* and 5*B*, on pages 468–471 of the same article. Distances along the abscissa measure degrees from the fovea, and distances along the ordinate measure sensitivity in terms of foveal sensitivity. The broken line represents the right eye and the straight line the left eye.

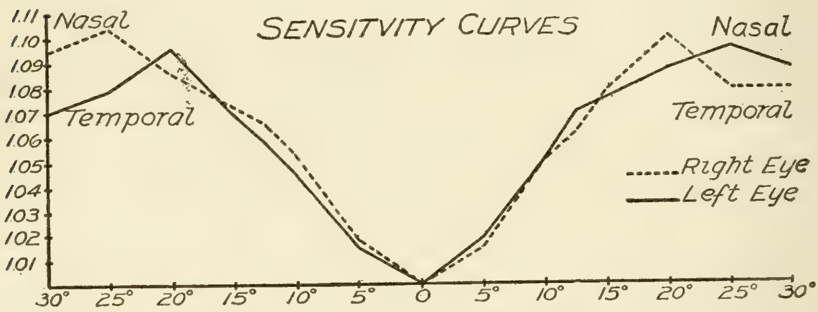

FIG. 5. Curves of Sensitivity for Different Parts of the Retina.

These curves show that the sensitivity of the retina increases as the distance from the fovea increases, as far as 25 degrees, when the sensitivity again begins to decrease. However the region of greatest sensitivity is less than 10 per cent. greater than that of the fovea. The sensitivity of the nasal side is greater than that of the temporal side, except at 20 degrees where the reverse is the case.[9] Comparing these curves with those for reaction times it seems that reaction time is not closely related to the sensitivity of the retina.

2. Ordinary observation teaches that *visual acuity* diminishes rapidly as we pass from the fovea toward the periphery of the retina. The method generally used in testing for acuity is to determine the distance at which certain letters may be read, or to determine the

[9] According to Schön, there is a marked difference between the light sensitivity of the nasal and the temporal halves of the retina. He found that a point 45 degrees from the fovea on the temporal side required for the production of a certain degree of brightness, three times as much light as the corresponding point on the nasal side. See Schön, *Arch. f. Ophth.*, Bd. **22**, Abth. 4, 49, 1876; Bd. **24**, Abth. 1, 27, 1878. Guillery, in ''Vergleichende Untersuchungen über Raum-, Licht- und Farbensinn in Zentrum u. Periphery der Netzhaut,'' *Zeitschr. f. Psychol. u. Physiol. des Sinnesorgane*, **12**, 243–274, 1896, considers the temporal half of the retina less sensitive than the nasal half, but the difference he describes as much less than that found by Schön.

distance two lines must be apart in order to be distinguished as two. The curve of visual acuity for different regions of the retina is shown in Fig. 6, and is adapted from one by Koester.[10] The abscissa measures the distance from the fovea in degrees. The scale for the ordinates is not given. The righthand curve (broken line) represents the nasal side of the right retina, and the lefthand curve (solid line) represents the temporal side of the left retina. This curve has in a general way the same characteristics as the reaction time curve; at least the acuity diminishes as the reaction time increases. In dark adaptation the acuity of the periphery of the retina is greater than that of the fovea. It is interesting to note that the relation between acuity and reaction time might be shown by repeating the reaction experiment at night or with the dark-adapted eye to see whether the reaction-time curve would reverse itself as the acuity curve does.[11]

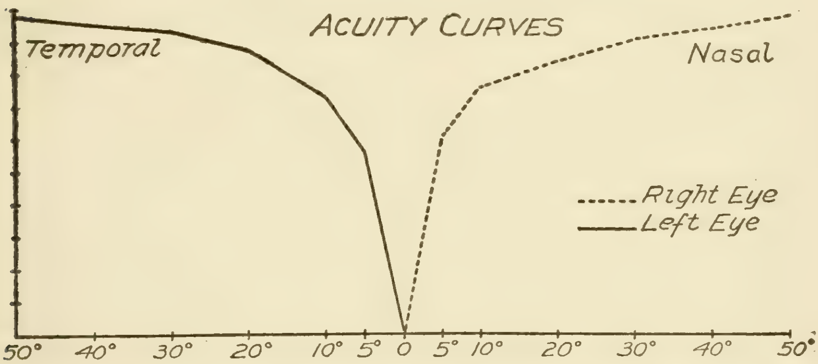

FIG. 6. Curve of Visual Acuity for Different Parts of the Retina. (Adapted from Koester.)

3. The relative speed of reaction upon stimulation of the temporal and the nasal sides of the retina may be compared with the *extent of the visual field* in these regions. In the average eye, the visual field on the temporal side corresponding to the nasal side of the retina, extends at least to 90 degrees, while the nasal side of the visual field, corresponding to the temporal side of the retina, is cut off by the nose at about 60 degrees. An average of the differences

[10] "Über Stäbchen- und Zapfensehschärfe," *Centralbl. f. Physiol.*, **10**, 433–436, 1896. The direction of the curves by Koester has been reversed, they are marked in terms of the retina instead of in terms of the visual field, and the scale upon the ordinate has been reduced.

[11] The distribution of the rods and cones throughout the retina might be considered in connection with the reaction time curve, but as acuity is considered to be very closely related to the arrangement of the rods and cones, this latter physical characteristic of the retina will not be discussed.

in reaction time between the temporal and the foveal position for both eyes and for both hands, and an average of the same for the nasal and foveal positions, shows that the difference for the nasal side is approximately two thirds of that on the temporal side. This means that the nasal side gives a quicker reaction by about one third than the temporal side.

4. Exner[12] and Bellarminow[13] have found that *flicker* is more marked in the periphery than in the center of the visual field, and have inferred from this that the intensity of the sensation produced by a certain stimulus is greater there. The latter has found also that flicker occurs more readily upon stimulation of the nasal half of the retina than upon the temporal half.

5. Within the last four years Stevens[14] has published three experiments upon ''The Peculiarities of Peripheral Vision,'' with special reference to the *apparent size of objects* and the *apparent speed of movement* in different regions of the visual field, and their relation to right-handedness. The last report contains a review of previous work on the subject. His general conclusions from his own work are that with few exceptions the right half of an extent in the visual field is overestimated, and that this estimation holds true for both eyes. Such an extent would cast its image upon the left corresponding halves of the two retinas and the impulses set up would reach the left occipital cortex. This predominance of the right half of the visual field does not appear in a study of the sensitivity and acuity curves of the different retinal areas, but in these cases the nasal seems to surpass the temporal of each eye.

IX. Conclusions

1. With the exception of subject *P* at 3 degrees the reaction time increases as the distance from the fovea is increased.

2. The nasal side of the retina gives a faster reaction than the temporal side, in nearly every case when the times for the two hands are combined.

3. This relation between the temporal and the nasal side of the retina agrees in general with the extent of the visual fields corresponding to the temporal and the nasal side of the retina; with the acuity relations of these two sides and with the relations of retinal sensitivity; it also agrees with the appearance of flicker upon the two halves of the retina.

[12] *Sitzungsb. d. k. Akad. d. Wissensch.*, Wien, Bd. **58**, Abth. 2, s. 601, 1868.

[13] *Archiv. f. Ophth.*, Bd. **35**, Abth. 1, s. 25, 1889.

[14] *Psych. Rev.*, **15**, 69 ff., 1908; **15**, 373 ff., 1908. Stevens and Ducasse, ''The Retina and Righthandedness,'' *Psych. Rev.*, **19**, 1–31, 1912.

CHAPTER VI

OTHER TIME RELATIONS

THIS chapter will deal with several problems: (I.) The Relation between the Speed of Reaction of the Right and Left Hands. (II.) The Relation between the Speed of Reaction to Monocular and Binocular Stimulation. (III.) Homolateral and Contralateral Time Relations between Hand and Eye, that is, a comparison of the reaction time of the right hand to stimulation of the right eye and of the left eye, and *vice versa.* Only the first of these problems directly concerns our investigation proper. The other two are closely related and seem to be of sufficient interest to be included in this chapter.

I. RELATION BETWEEN THE REACTION TIME OF THE RIGHT AND LEFT HANDS

In addition to the differences between retinal areas discussed in Chapter V, it is necessary to find whether there is a difference in the speed of reaction of the right and left hands, for if there is such a difference it must be eliminated, as suggested at the close of Chapter III. The results of previous experimental work in this field are conflicting, so that it is necessary to repeat the test under the conditions of this experiment, in order to draw conclusions which shall be valid, at least for the subjects taking part in this research.

According to Cattell,[1] the duration of the reaction time is the same for the right and left hands. In the case of light stimuli, his average time for the right hand was 146 sigma and for the left hand 147 sigma. Kiesow[2] took a series of 50 reactions to sound with each of the five fingers of each hand. The reactions were of the sensory type, the source of sound was the Wundt sound hammer and the time was recorded with the Hipp chronoscope. The sound is described as of moderate intensity. The following figures show the averages for the fingers of each hand with the mean variations. I have calculated the averages of the right and left hands together with the mean variation and the probable error of the averages.

[1] "On Reaction Time, etc.," *Memoirs Nat. Acad. Sci.*, 7, 395 and 411, 1893.

[2] "Beobachtungen über die Reaktionzeiten momentaner Schalleindrücke," *Archiv f. d. ges. Psych.*, **16**, 353–375, 1910.

	Right Hand			Left Hand			
	Av.	M. V.	P. E.	Av.	M. V.	P. E.	Diff.
1st finger	171.7	19.5		172.4	16.8		
2d finger	175.2	17.0		178.0	13.5		
3d finger	166.8	18.5		173.8	13.0		
4th finger	170.5	16.3		176.2	16.5		
5th finger	175.3	19.5		168.7	18.2		
Average	171.9	18.2	1.0	173.8	15.6	0.8	1.9

The M.V. is obtained by averaging the mean variations of the different fingers. The P.E. is obtained according to the formula

$$P.E = \frac{.8453\ M.V.}{\sqrt{n}}$$

in which $n = 250$. The difference is quite small but Kiesow believes that there is a distinction between the reaction times for the two hands and considers the problem worthy of further study. It is interesting to notice that this difference is just about the same as that found by Cattell, although the latter considered the times for the two hands the same.

Kiesow, in the same publication reports a more extended research upon the same problem. The reactions were of the motor type and the sound he describes as strong. Seven persons acted as subjects. The averages are given by Kiesow in series of ten for each hand, but I have combined them into general averages. The averages for the first three subjects include 1,000 reactions each, for the fourth 500 and for the last three 250 reactions each. The first four subjects were right-handed and the last three were left-handed. The letters r.h. and l.h. following the name of the subject designate the right- and the left-handed subjects respectively. These averages are given in Table XIII. In the last column are given the differences between the two hands, the minus sign indicating that the left hand is faster.

This table shows clearly that in the right-handed persons, the right hand is somewhat faster than the left hand; in the left-handed

TABLE XIII

Reaction Time of Right and Left Hand to Auditory Stimuli (Kiesow)

	Right Hand		Left Hand		
Subject	Av.	M. V.	Av.	M. V.	Diff.
J. C. (r. h.)	105.3	5.3	109.3	5.5	4.0
C. S. (r. h.)	105.2	6.2	109.8	6.4	4.6
L. B. (r. h.)	105.6	5.3	107.6	6.0	2.0
M. P. (r. h.)	108.0	6.9	111.1	6.5	3.1
R. (l. h.)	116.5	8.0	107.5	7.0	—9.0
M. M. (l. h.)	113.4	8.5	110.4	8.6	—3.0
P. E. (l. h.)	114.8	8.5	111.6	7.8	—3.2

persons the left hand is somewhat faster than the right. Kiesow mentions the work of Herlitzka,[3] who found somewhat higher results, by a slightly different method. The differences were in the same direction as those found by Kiesow.

A set of experiments upon the reaction time of the right and left hands was made in this research, in order to discover whether a difference between the time of the two hands would be a disturbing factor, which would have to be eliminated before the data could be properly interpreted. One subject, *P*, is left-handed and the others are right-handed. The tests consisted of reactions to light made as described in Chapter IV., except that both eyes were open and the light stimulus arranged to strike the center of vision of both eyes equally. According to the conduction paths as they have been outlined, association between the hand and the center of vision of each eye would be through equally direct paths, and any difference in time between the reaction of the two hands would be due to other causes such as training. Table XIV. gives the results of these experiments on three subjects. The figures for each hand are the average of 100 reactions for subjects *T* and *A*, and of 200 for subject *P*. The reactions were taken in series of 10 alternately with the right and left hand.

TABLE XIV

Reaction Time of Right and Left Hands when Both Eyes are Stimulated

Subject	Right Hand Av.	Right Hand M. V.	Left Hand Av.	Left Hand M. V.	Diff.
T (r. h.)	184.4	9.9	184.7	5.2	0.3
P (l. h.)	160.4	4.4	162.5	5.2	2.1
A (r. h.)	178.9	6.5	177.2	5.0	—1.7

The differences between the reaction times of the right and left hands are even smaller than those found by Kiesow. In subject *P* who is left-handed, the right hand is faster than the left by 2.1 sigma; in subject *A* who is right-handed, the left hand is slightly faster, 1.7 sigma; and in the case of subject *T* there is no real difference.

When only one eye is stimulated on its fovea the differences are larger and the relations are slightly changed. In Table XV. these averages for the three subjects are given with their mean variations. Each figure is an average of 400 reactions for subjects *T* and *P*, and of 200 reactions for subject *A*.

[3] "Ricerche cronografiche sui movimenti volontari bilaterali," *Arch. di fisiol.*, **5**, 277–284, 1908.

TABLE XV

REACTION TIME OF RIGHT AND LEFT HANDS WHEN ONE EYE IS STIMULATED

Subject	Right Hand Av.	Right Hand M. V.	Left Hand Av.	Left Hand M. V.	Diff.
T (r. h.)	200.5	8.4	202.1	8.8	1.6
P (l. h.)	177.5	3.8	171.7	5.2	—5.8
A (r. h.)	193.9	9.4	188.5	12.5	—5.4

Inspection of this table shows that subject *P*'s *left* hand gives the quicker reaction when one eye is stimulated, the reverse of that shown in Table XIV. For the other two subjects the differences are increased in the same direction. It is these differences upon monocular stimulation which are of significance in this investigation, because all the regular experiments are carried on under monocular stimulation. No explanation is offered for the change in the direction of the differences as the stimulus falls upon one eye or two eyes.

II. Relation between the Speed of Reaction to Monocular and Binocular Stimulation

Table XVI. shows the difference in reaction time when the stimulus strikes only one eye and when it strikes both eyes.

TABLE XVI

REACTION TO MONOCULAR AND BINOCULAR STIMULATION

Subject	One Eye Av.	One Eye M. V.	Both Eyes Av.	Both Eyes M. V.	Diff.
T	201.3	8.6	184.6	5.0	16.7
P	174.8	6.0	160.4	5.0	14.4
A	191.2	11.0	178.1	6.0	13.1

The last column in the table shows the differences. The data for this study are taken from Tables I., II. and XIV. The reactions with the right and left hands to foveal stimulation are combined separately for each eye, making a total of 800 reactions for each eye, except in the case of subject *A*, with a total of 400 reactions. It is the averages of these 800 reactions which appear in Table XVI. under the column marked "One Eye." The figures under the column marked "Both Eyes" are the combined averages of the right and left hands taken from Table XIV. and are made up of 200 reactions for subjects *T* and *A*, and 400 reactions for *P*.[4]

[4] The tests with binocular stimulation were made between the tests recorded in Tables I. and II. This arrangement would equalize the practise effect in the two kinds of tests compared in Table XVI. For this reason the foveal times in

There seems to be a clear difference in the speed of reaction when one eye is stimulated and when both eyes are stimulated, and the differences are almost the same for the three subjects, 16.7, 14.4 and 13.1. It is difficult to explain this difference as due to the retinal apparatus. It has been found that the apparent increase in brightness is about one tenth when the two eyes observe an object instead of one eye. But such a small difference in brightness value would offer no explanation for the difference in reaction time. According to the work of Froeberg, a reduction of 50 per cent. in the brightness of a stimulus makes a difference of only a few sigma in the reaction time. Ordinary observation shows that there can not be a great difference in the brightness under monocular and binocular vision, or there would be a difference in the brightness of parts of our visual field, according as an object fell within the range of binocular vision or only of monocular vision. One might attribute the whole difference to the unusual conditions imposed by monocular vision in an ordinary person. But the eye screen employed allows the unused eye to remain open and after a very short time seems to give no discomfort. As stated in Chapter IV., the subject was often unable to tell which eye was closed and which was open without feeling with his hands.

If the difference in the speed of reaction upon stimulation of both eyes or one eye, is a central one, that is, if it is due to a difference in the speed of conduction through the brain centers, then the dynamogenic effect of simultaneous stimuli might afford an explanation.[5] Each fovea is represented on each occipital cortex, so that a stimulus striking the fovea of one eye would react upon both visual

Tables III. and IV. are not averaged in with those in Tables I. and II. As subject *A* appears only in Table I., her reactions to binocular stimulation followed those to monocular stimulation. Therefore, if the practise effect were appreciable in the case of *A*, it would make her differences in Table XVI. too high.

[5] Dynamogenesis is discussed by James and others. (See James, "Principles of Psych.," **2**, 379 ff., 1908.) Feré in "Sensation et Mouvement," 1889, describes a large number of experiments to show the dynamogenic effect produced through the difference senses. In the cases cited by Feré, the reinforcing stimuli are received through sense organs other than those to whose stimulation the subject is reacting, and the effect is explained as a diffusion or overflow of the nervous energy. This reinforcement would occur, the writer believes, to an even greater extent, where both stimuli act directly on the same sense organ and hence on the same cerebral center.

The latest work on dynamogenesis is that of Todd, "Reaction to Multiple Stimuli," ARCHIVES OF PSYCHOLOGY, No. 25, 1912, in which the author finds considerable reinforcement effect in the case of visual, auditory and tactual stimuli.

centers in the cortex. A stimulus falling on the fovea of each eye would act as a double stimulus upon each visual center. Since it is one of the generally accepted qualities of a center, that the time lost therein decreases as the intensity of the stimulus increases, the greater energy set free by the double stimulus would account for the decrease in reaction time.

III. Homolateral and Contralateral Time Relations between Hand and Eye

The third problem to be considered in this chapter is that of the time relations between hand and eye, on the same side of the body, and on opposite sides of the body. There is a general belief that the eye and hand, foot, etc., on the same side of the body are more closely related than the eye and those members on the opposite side of the body. The following quotations are taken from Gould's[6] book on "Right-handedness and Left-handedness."

> Right-handedness is necessarily bound up with right-eyedness, with right-earedness, with right-footedness. . . . We are usually as right-eyed as we are right-handed. The reason for this general choice of the arm and hand, of eye, of ear, and of leg and foot for conjoined expert tasks is easily recognized. It insures a speedier and more accurate synthesis of the cerebral functions which must be coordinated into a single act and result. The independence of the two halves of the brain makes it necessary that the bodily organs most commonly acting together and most interdependent should be incited and controlled by the cerebral centers in close contiguity and in one side of the brain. There is a measurable slowness of nerve current transmission, . . . and even if the connecting links between the two brain-halves were much more intimate and numerous and short than they are, rapidity and accuracy of correlation and unification in willed act would be impaired, and the safety and decision of the entire organism imperiled, if one or two of the coacting centers were in opposite hemispheres (pp. 30–31).
>
> Physiologically, therefore, the reason why an infant puts forth the right hand to grasp objects is because the right eye is the one which is nearest perfect visually, anatomically, or optically (p. 44).

The preceding quotations refer to the general relation between members on the same side of the body. The following studies relate to the particular relations existing between the sensory and motor apparatus in the same member.

Numerous investigations have been made to determine whether the reaction time is faster when the stimulus is applied to the reacting member, and the conclusions do not all agree. The experiments of Exner[7] showed that upon electrical stimulation of the

[6] "Right-handedness and Left-handedness," 1908.

[7] See Exner, "Hermann's Hanb. der Physiol.," 2, Pt. 2, 264, 1879.

hand, the reaction was 10 sigma longer when the stimulus was applied to the reacting hand. Sanford[8] repeated the experiments of Exner, on several subjects, using the induction shock as a stimulus. He found just the opposite results from those of Exner, that the times were faster in every case when the stimulus was applied to the reacting hand. The case of stimulation and reaction by right hand gave times faster than those upon stimulation of the left hand and reaction by the right hand, by 14.3, 2.4 and 3.3 sigma for the three subjects. When the stimulus is applied to the left hand, the reaction by the left hand is faster than that of the right hand by 1.7, 16.9 and 3.7 sigma for the three subjects.

Cattell[9] found the reaction time, when the stimulus was applied to the reacting hand to be 21.1 sigma shorter for one subject and 9.4 sigma shorter for another subject, than when the other hand reacted. He says:

> This might be expected, as the movement is a natural reflex—a person will without reflection withdraw the hand when it touches a hot surface.

The work about to be reported is more analogous to the first study quoted, the relation between members on the same side of the body. Table XVII. shows the relation between hand and eye on the same and opposite sides of the body, for subjects *T* and *P*.

TABLE XVII

HOMOLATERAL AND CONTRALATERAL RELATIONS BETWEEN HAND AND EYE

	Right Eye				Left Eye			
	Right Hand		Left Hand		Right Hand		Left Hand	
Subject	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
T (r. h.)	193.2	7.7[10]	197.0	6.6	192.8	7.5	195.9	8.1
P (l. h.)	178.4	3.8	173.7	3.8	181.1	3.5	173.0	4.8

Each figure is an average of 400 reactions, being the average time for the foveal position in each of the Tables I., II., III. and IV. For subject *T,* the faster times are obtained where the right hand reacts, regardless of the eye, in fact the time is a trifle shorter for the left eye, that is the eye *opposite* to the reacting hand. For subject *P,* the faster times occur where the left hand reacts, the time being practically the same whether the stimulus falls upon the right or the left eye. The conclusion from this table must be that, as far

[8] "On Reaction Times when the Stimulus is Applied to the Reacting Hand," *Am. J. Psych.,* **5**, 351–355, 1892–1893.

[9] "On Reaction Times, etc.," *Memoirs Nat. Acad. Sci.,* **7**, 410, 1893.

[10] These deviations are the averages of the deviations given in Tables I., II., III., IV., for the given positions.

as the reaction times are concerned, the relation between hand and eye on the same side of the body is no closer than that between hand and eye on opposite sides of the body.

One would not expect an especially close connection between hand and eye on the same side of the body, as a result of the anatomical relations between them. For according to the arrangement of the optic fibers as they were discussed in Chapter III., each eye (speaking of the central vision only, which the figures in Table XVII. represent) is equally represented on each cerebral cortex. Even considering peripheral vision, one would not expect one eye, as a whole, to be related to a given hand, but the left side of each eye would be most directly connected with the right hand, and the right side of each eye with the left hand. (These relations may be easily seen by glancing at Fig. 1.) This means that an object in the right visual field would cast its image upon the left side of each retina, and would be most directly connected with the right hand, or that hand nearest to the object seen. Even if the right eye were defective, a stimulus coming from the right visual field would still find its most direct course to the right hand, as the stimulus would fall upon the temporal side of the left eye as well as upon the nasal side of the right eye. To speak physiologically therefore, better vision in the right eye would not account for right-handedness, as Gould states in the work quoted. On the contrary, the anatomical relations of the separate halves of the retina with the occipital cortex and this in turn with the hands, would make the right hand most directly associated with objects in the right visual field and *vice versa,* but not with the right or the left eye as a whole. Right-handedness would not necessarily mean greater development of the left occipital cortex and the consequent evolution of the left hemisphere as the dominant one (see Gould, *loc. cit.,* p. 43).[11]

[11] Stevens, quoted in Ch. V., in "The Retina and Right-handedness," shows that in the right half of the visual field in right-handed persons, objects appear larger than in the left half of the visual field. On these facts he bases his theory of right-handedness. This theory conforms more closely to the anatomical arrangement of the optic fibers than the theory proposed by Gould. The reactions by the right hand to stimuli coming from the right visual field (acting upon the nasal side of the right eye and the temporal side of the left eye) are faster in subject T by 0.6 sigma, than to those coming from the left half of the visual field, when reacted by the left hand. In subject P, the difference is 5.5 sigma in the opposite direction, that is, reaction by the left hand to stimuli coming from the left half of the visual field is quicker than reaction to stimuli from the right half of the visual field by the right hand. This result is interesting since subject T is right-handed and P is left-handed. Although the difference

The case where the stimulus is applied to the reacting member is quite different from that of vision. By the anatomical arrangement of the sensory and motor fibers, that is, the total decussation of both the motor and sensory fibers leading to and from the limbs, the muscles of one hand would be most directly associated with the sensory nerve endings in that hand, and a reflex over that arc would be most simple.

Conclusion.—To conclude from the data set forth in this chapter, there is a difference in the reaction time of the right and left hands, in the subjects tested, which must be accounted for in the calculation of central time as proposed in the preceding chapters, and which will be taken up in Chapter VII. Further, we may conclude that when the stimulus acts upon both eyes the reaction time is faster than when only one eye is stimulated, probably the dynamogenic effect of the double stimulus. And from the last section of the chapter we may conclude that the relation between hand and eye on the same side is no closer, as far as reaction time is concerned, than between hand and eye on opposite sides of the body.

in the case of T is too small to be significant, the tendency of the two subjects shows for reaction time, what Stevens showed for size of objects, a preference for the right half of the visual field in right-handed persons and for the left half of the visual field in left-handed persons.

CHAPTER VII

SYNAPSE TIME

The purpose of this investigation is to find a means of measuring the time lost in the transmission of an impulse through a synapse within the human nervous system, and to obtain an approximate measure of this lost time. The preceding six chapters have prepared the way for its discussion.

1. They show (Chs. I. and II.) the difficulties and errors which have occurred in the previous studies of conduction through nerves and nerve centers; they show also that in the living human being, the reaction-time method is the only means by which the problem can be approached.

2. They show (Ch. II.) that the problem may be solved by comparing the reaction time over two nerve paths identical except that the one must cross from one side of the brain or cord to the other, thereby involving one additional synapse or system of synapses.

3. The conduction paths involved in stimulation of the eye, and reaction to this stimulus by the hand are shown (Ch. III.) to fulfill these requirements by virtue of the peculiar division of the optic fibers in passing through the optic chiasm. Chapter III. analyzes these conduction paths and points out certain preliminary problems which the use of these paths involves.

4. Chapter IV. describes the apparatus and methods which are employed in the study of these paths.

5. Chapter V. discusses the first of the preliminary problems, the difference in the quality of the different parts of the retina, showing itself in a difference in the speed of reaction to stimulation of these parts, and shows that account must be taken of this difference.

6. Chapter VI. considers the second of the preliminary problems, the difference in the reaction time of the right and left hands. It shows that when one eye only is stimulated, there is a difference in the reaction time of the two hands, which would, unless removed, probably obliterate the differences in time due to synapses. In addition, this chapter takes up two other problems not directly associated with the main probem, but having a certain intrinsic interest.

We have found that the *right* hand is most directly associated

with the *right* retina on its *nasal* side (*A*, Fig. 1) and with the *left* retina on its *temporal* side (*A′*, Fig. 1); also that the *left* hand is most directly associated with the *right* retina on its *temporal* side (*B*, Fig. 1), and with the *left* retina upon its *nasal* side (*B′*, Fig. 1). These four conduction paths from eye to hand will be called direct, as they are the ones which do not involve the association tract between the two motor centers.

Those paths which do involve this association path between the motor centers, will be called indirect. They are the paths traversed when the *right* hand reacts to a stimulus upon the *temporal* side of *right* retina, and upon the *nasal* side of the *left* retina; also when the left hand reacts to a stimulus upon the *nasal* side of the *right* eye, and the *temporal* side of the *left* eye. To put these conduction paths into an abbreviated form so that they may be seen at a glance, we will use R.E. and L.E. for right and left eye respectively, R.H. and L.H. for the right and left hand respectively and Temp. and Nas. for the temporal and the nasal sides of the eye, and arrange them as follows:

$$\text{Direct}\begin{cases}\text{R.H.}\begin{cases}\text{R.E. Nas.}\\ \text{L.E. Temp.}\end{cases}\\ \\ \text{L.H.}\begin{cases}\text{R.E. Temp.}\\ \text{L.E. Nas.}\end{cases}\end{cases}\qquad\qquad \text{Indirect}\begin{cases}\text{R.H.}\begin{cases}\text{R.E. Temp.}\\ \text{L.E. Nas.}\end{cases}\\ \\ \text{L.H.}\begin{cases}\text{R.E. Nas.}\\ \text{L.E. Temp.}\end{cases}\end{cases}$$

Comparison of any one direct path with any one indirect path will have in it as a disturbing factor either the difference in time due to the retinal area stimulated, or to the difference between the reaction time of the two hands. For example, to compare the first line of the direct with the first line of the indirect paths in the chart, and put them into the form of an equation, we have

$$\text{R.H. R.E. Nas.} < \text{R.H. R.E. Temp.}$$

No difficulty arises from the hands since both sides of the equation contain R.H. But one side contains Nas. and the other Temp. and as the temporal side of the retina, independently of the hand associated with it, gives a longer reaction than the nasal side, this would affect the result. Compare the first line of the direct with the third line of the indirect paths and we have

$$\text{R.H. R.E. Nas.} < \text{L.H. R.E. Nas.}$$

In this case Nas. appears on both sides of the equation, while the difference between the right and left hands remains as the disturb-

ing factor. But when the whole four direct paths are combined into an equation with the four indirect paths we have

$$\left.\begin{array}{l}\text{R.H. R.E. Nas.}\\ \text{R.H. L.E. Temp.}\\ \text{L.H. R.E. Temp.}\\ \text{L.H. L.E. Nas.}\end{array}\right\} < \left\{\begin{array}{l}\text{R.H. R.E. Temp.}\\ \text{R.H. L.E. Nas.}\\ \text{L.H. R.E. Nas.}\\ \text{L.H. L.E. Temp.}\end{array}\right.$$

Now Nas and Temp. and R.H. and L.H. are equally distributed on each side of the equation. The averages of these two sides of the equation should give results in which the right hand offsets the left and the nasal side of the retina offsets the temporal side, so that both the disturbing factors are eliminated. It is the possibility of thus combining the several factors in question that makes these paths especially fitted for the present study. Solving this equation, then, should give a difference which is due to the time consumed in the passage of the impulse across the synapse or synapses involved.[1]

$$\left.\begin{array}{l}\text{R.H. R.E. Temp.}\\ \text{R.H. L.E. Nas.}\\ \text{L.H. R.E. Nas.}\\ \text{L.H. L.E. Temp.}\end{array}\right\} - \left\{\begin{array}{l}\text{R.H. R.E. Nas.}\\ \text{R.H. L.E. Temp.}\\ \text{L.H. R.E. Temp.}\\ \text{L.H. L.E. Nas.}\end{array}\right. = \text{Synapse Time.}$$

Table XVIII. gives the values obtained from the solution of this equation, for the four distances from the fovea, 45, 30, 10 and 3 degrees.

TABLE XVIII

SYNAPSE TIME

Subject		45 Deg.	45 Deg.*	30 Deg.	10 Deg.	3 Deg.	Av.
	Av.	—1.5	0.5	4.5	6.2	6.2	**5.6**
	M.V	*13.2*	*13.6*	*6.9*	*7.9*	*6.9*	
	P.E.	*1.78*	*2.55*	*0.92*	*1.05*	*0.92*	
	Av.	1.7	5.7	7.0	4.6	6.4	**6.0**
	M.V.	*10.6*	*13.2*	*7.2*	*8.2*	*5.0*	
	P.E.	*1.41*	*2.48*	*0.96*	*1.09*	*0.67*	
	Av.	—1.6					
	M.V.	*7.7*					
	P.E.	*1.02*					
	Av.			2.0			
	M.V.			*8.3*			
	P.E.			*1.50*			

[1] The difference thus obtained would obviously include the length of nerve between the two motor areas in the two hemispheres. If the speed of transmission of the nerve impulse be taken as 30 meters per second, then in one sigma it would travel 3 cm. Considering the length of these fibers to be 6 cm., their conduction time would be 2 sigma, which would be a constant quantity.

Four subjects are represented in the table, *T* and *P* when the temporal and nasal position are each 45, 30, 10 and 3 degrees from the fovea; subject *A* when they are 45 degrees from the fovea; and subject *M* when they are 30 degrees from the fovea. These distances are represented in the vertical columns of the table and the subjects in the horizontal lines. Two columns marked 45 degrees appear in the table. The second one marked with a star contains the differences obtained from a repetition of the first experiment at 45 degrees. It consisted of 5 series or 50 reactions upon each position, half the number which make up the figures in the first column of the table. The original averages for the figures in the first column are given in Tables I.–IV., and for those in the second column, in Table XI. This later work was repeated only on subjects *T* and *P*. As subject *A* served only for tests at 45 degrees, and *M* for tests at 30 degrees, values for these subjects appear in columns marked 45 and 30 degrees. The last column in the table requires further explanation and will be discussed later.

Each figure in the table (with the exception of those for subject *M* and those for *T* and *P* in the second column at 45 degrees) is based on 800 reactions,[2] while the whole table comprises about 10,000 reactions.

Consider first the differences obtained at 45 degrees from the fovea, given in the first column of Table XVIII. In subject *T* the difference is a negative one, that is, the path which has been described as direct gives the longer reaction time by 1.5 sigma. In subject *P* the difference is 1.7 sigma in the positive direction, the direct path faster; in subject *A* the difference is also negative by 1.6 sigma. In these three cases the mean variations are high and the differences fall within the limits of the probable errors, 1.78, 1.41 and 1.02. The negative values for the differences are therefore not significant, since the chances are small that they are real. The difficulty and uncertainty, described by the subjects in reaction to stim-

[2] The differences given in Table XVIII. were not obtained by subtracting one grand average of 400 reactions from another similarly obtained, but they are the average of 40 differences between series of 10 reactions each. And the mean variations are the variations of these series differences from the average difference. By this method of calculating the mean variation, the P.E. is obtained according to the formula

$$\text{P.E.} = \frac{.8453 \text{ M.V.}}{\sqrt{n}}$$

where n equals 40. By a different method of calculating the M.V. described in Ch. V. (note, p. 50), the P.E. would appear considerably lower, as n would equal 400.

ulation of this extremely peripheral position, are supported by these large mean variations. The fact that a valid difference due to the synapse time fails to appear in this instance, supports rather than detracts from the results obtained upon the less eccentric positions. It has been suggested that the difference would be less in the cases which I have designated direct, because it is customary to react on the side from which the stimulus comes, whatever the nature of the stimulus may be. But just where this cause should act most forcibly, at an extremely eccentric position, the differences fail to appear, and at 3 degrees where the head and body are turned almost directly toward the stimulus, a real valid difference occurs. Therefore considering the unreliability of the figures from which the values at 45 degrees are obtained, and the generally unfavorable conditions attendant upon reaction to stimulation of such an eccentric position, the writer feels justified in excluding these averages from the calculation of the average synapse time.[3]

The second column in the table, marked with a star, was partly explained above. In these tests the revolving chair was used instead of rotating the eyes in their sockets. In this case the differences shown in the table are both positive, 0.5 and 5.7 sigma, but again the probable errors are so high as to make the difference, for subject *T* at least, of no validity. It is upon the less eccentric positions that I consider the differences by far the more reliable, aside from my knowledge of the probable errors of the figures.

The third column in the table shows the differences at 30 degrees. For subject *T* the direct path is 4.5 sigma faster than the indirect path. As the probable error is less than one (0.92), the chances are less than 1 out of 1,000 that the difference is not a real one. For subject *P* the direct path is 7.0 sigma faster than the indirect one, with a P.E. of only 0.96. The tests upon subject *M* were at 30 degrees from the fovea and the differences are given in this column. Only five series of reactions were taken upon this subject, as stated in Chapter V. The difference is 2.0 sigma in favor of the shorter path, but the P.E. of 1.50 is too high to make this difference very reliable. However the tendency toward faster times over the direct paths supports the results from the other two subjects.[4]

[3] It was the original intention of the experimenter not to include the reaction time to stimulation at 45 degrees from the fovea, in the study of synapse time, for the reasons given above. The work was done on this position in order to make the curve of reaction time to stimulation of different retinal areas more complete. Reactions were also attempted at 60 degrees, but tests at this distance were deemed unnecessary.

[4] Experience with several subjects convinced the writer that such an intensive

At 10 degrees, in the fourth column of the table, subject *T*'s time is faster over the direct path by 6.2 sigma, with a P.E. of 1.05. For subject *P,* the difference is 4.6 sigma in favor of the direct path with a P.E. of 1.09. Although the P.E. is slightly over 1 in both these cases, the chances are still less than 1 out of 1,000 that the difference is not a real one.

The fifth column of the table shows the synapse time when the stimulus is applied at 3 degrees from the fovea. The direct path for *T* is 6.2 sigma faster than the indirect, exactly the same value found at 10 degrees, and the P.E. is lower, 0.92. For subject *P* the direct path is 6.4 sigma faster than the indirect, with a P.E. of 0.67. This difference for *P* is just about the same as that found for *T* at 3 degrees. The similarity between the results for the two subjects and the low probable errors further confirm the statement made above, that the less eccentric the position the more valid is the result.

The last column in the table gives the average synapse time for subjects *T* and *P,* including in the average the differences found at 30, 10, and 3 degrees.[5] There is no particular advantage in computing the average of these figures, except to make a general comparison with the results of other experimenters, for an average does not show the great similarity in the results for the two subjects in the three distances from the fovea. The values for the synapse time obtained by experimenting upon the different eccentric positions agree very closely. The reliability of these figures must be raised considerably above that shown by the P.E. when one considers that in three different situations with weeks intervening between the separate experiments, such similar results have been obtained.

If the arguments in the introductory chapters of this work, which are based on the most generally accepted theories of nerve action and upon the results of recent researches in this field, are correct, we have determined the time required for the nerve impulse, whatever its nature may be, to traverse one synapse or system of synapses. Moreover this has been done without using any of the conflicting calculations of the speed of conduction in the plain nerve.

study as the present one must be limited to one or two subjects trained in reaction time work and familiar with the requirements of psychological experiments. In addition to this, they must have an immense amount of time at the disposal of the experimenter. For these reasons the main conclusions will be drawn from the data upon subjects *T* and *P*.

[5] The differences at 45 degrees have been excluded from the averages for the reasons given above. The following figures give the averages including the figures in columns 1 and 2, that is, the two sets at 45 degrees: *T* 3.2 and *P* 5.1 sigma.

No work has been done upon human beings, where the time value has been limited to one nerve unit, and only a few researches of a similar nature have been made upon animals. The results of Wundt's experiments upon the spinal cord of a frog were given in Chapter II (p. 20). He stimulated the sensory nerve leading from the gastrocnemius muscle at its entrance into the cord and recorded the time of the reflex muscular response in the muscle on the same side of the body. He then stimulated the sensory nerve on the other side of the body and recorded the latent period of the same muscle as before. In the latter case the reflex was crossed, that is, the excitation had to pass through the cord by means of a short association fiber[6] to the other side where it connected with the motor nerve leading to the muscle. The difference between the lost time of the two paths was 4 sigma, which Wundt attributed to the one additional synapse.

Sherrington found the latent time of the flexion reflex in the dog for a strong stimulus[7] to be 30 sigma. Assuming that the speed of conduction in the nerve is 30 meters per second, and allowing a certain time for mechanical latency in the muscle, he accounted for 27 sigma of this total amount. Three sigma, therefore, would be the time lost within the center in the spinal cord, and if one synapse were involved in this center, its time would be 3 sigma.

Comparing the results of Wundt on the frog, where the mechanism is comparatively simple, and those of Sherrington on the dog, where the nervous system is more complex, with those obtained in the present work upon the human cortex, we find for the frog, 4 sigma, for the dog 3 sigma and for man, 5.6 and 6.0 sigma as the time lost in a synapse. But if we make allowance for the time of transmission over the length of fiber between the two motor areas in the human brain, as stated above (p. 68, note), we have 3.6 sigma and 4.0 sigma as the time lost in the synapse for man. Therefore to judge from

[6] The short association fibers connecting one side of the cord with the other side are described by Howell (''Textbook of Physiol.,'' 1908. See Fig. 68) as ''commissural cells whose axons pass chiefly through the anterior commissure to reach the anterior column of the other side.''

These fibers are shown diagrammatically by Starr (''Atlas of Nerve Cells,'' 1896, 24, Fig. 3). He says, ''A few collaterals pass to the median gray matter and terminate in brush-like expansions about the commissural cell whose neuraxon passes to and terminates about the motor cell of the opposite side.''

[7] It may be well to repeat here that the light stimulus used in our work was rather strong, and that the central time would, on this account, be about its minimum. The light was made strong intentionally so that later work could be done with reduced intensities, for the purpose of comparing the synapse time under these varying conditions.

this evidence, we may say that, as far as time value is concerned, a synapse is a synapse whether it be in the simplest or the most complex nervous organization. The same speed of transmission over the plain nerve has been found in these cases. But the similarity in the case of the synapse is far more significant for those who believe that the synapse is the critical point in the study of brain activity.

The conclusion to be drawn from this experiment is that the method outlined in the preceding chapters offers an opportunity for the measurement of the time lost in conduction over a synapse or system of synapses in human beings, that this time for two subjects is 3.6 and 4.0 sigma, and that it shows great similarity between the times found for conduction through synapses in lower animals and in human beings. With the possibility of measuring synapse time, those theories which attribute the effects of ether, chloroform, alcohol and various drugs to a blocking of synaptic conduction, and those which account for practise, memory, training, attention and the like by changes in the conductivity of the synapse may be subjected to an experimental test.

INTERFERENCE AND ADAPTABILITY

AN EXPERIMENTAL STUDY OF THEIR RELATION WITH SPECIAL REFERENCE TO INDIVIDUAL DIFFERENCES

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THE writer wishes to express his deep indebtedness to all of those who acted in the capacity of subjects, many of whom sacrificed for the sake of the experiment. He is under obligations to Professor Bergström, whose various articles suggested the use of cards as material. For the material in one of the discrimination reaction experiments he is indebted to Margaret Hart Strong and Dr. Hollingworth. For many suggestions as to methods of treatment and conduct of the experiments, thanks are due to Professor R. L. Woodworth and Professor E. L. Thorndike. For the suggestion of the study, for continual supervision and guidance of the work, the writer is most deeply indebted to Professor J. McKeen Cattell. For assistance in the final arrangement of material the author's thanks are due to his wife.

INTERFERENCE AND ADAPTABILITY

CHAPTER I

Introduction

1. *Statement of Problem*

The previous work upon interference of associations has given rise to some definite problems. Münsterberg asked whether a given association can function automatically, while some effect of a previous and different association with the same stimulus remains. Also in case the new association becomes automatic, what is the condition of the former association with the same stimulus? Does it disappear or can the two entirely different movements be connected with the same sensation complex, and either of them be called up independently?[1] Müller and Schumann worded their problems thus: "When a series of nonsense syllables has been learned until the first correct repetition is possible, and is then relearned to the same extent after a certain interval, will more repetitions be required if in the meantime the syllables of the series have been associated with another set of syllables?"[2] Bergström devotes his attention to the question whether the interference effect is equal to, greater or less than the practise effect.[3] Among the purposes of Bair's experiments was "to determine . . . the increasing amount of interference, first, when there is an alteration in the serial order of stimuli, and secondly, when there is an alteration of particular responses to particular stimuli associated by preceding practises."[4] These investigators have asked the following questions. Is there such a thing as true interference, and if so how can it be measured? What is the effect of a well-fixed association on the formation of a new one with the same stimulus, how great is this effect, and can it be overcome? When

[1] Hugo Münsterberg, "Gedächtnisstudien, Beiträge zur Experimentellen Psychologie," **1**, **4**, 70, 1892.

[2] Müller and Schumann, "Experimentellen Beiträge zur Experimentellen Untersuchung des Gedächtniss, *Zeitschrift f. Psych. und Phys. der Sinnesorgane*, **6**, 173, 1893.

[3] John A. Bergström, "The Relation of the Interference to the Practise Effect," *Am. Jour. Psych.*, **6**, 434, 1894.

[4] J. H. Bair, "The Practise Curve," *Columbia Univ. Contributions to Philosophy, Psychology, and Education*, **5**, **9**, 5, 1902.

the new association has become established what is the condition of the former? And finally, can both of them become automatic and either be called up without the appearance of the other?

This paper is concerned with the problem of individual differences in interference. It is concerned not primarily with the fact or amount of interference when two conflicting associations are opposed, but rather with the rapidity and extent to which the interference is overcome by a new association, and with the adaptation of the individual to the new association. This of course has special interest for the study of individual differences.

There are two phases of the problem to which the two experiments will apply. The first is the interference effect of a well-established and long-practised habit association of a series of simple stimuli with equally simple reactions. At the end of the long period of practise other reactions are substituted for the same stimuli or series of stimuli, and a measure is then obtained of the adaptability of the individual to the new association and of the interference effect of the previous association upon the new one. The second type is that in which two mutually opposing associations are alternated. The interference of the one against the other is measured by its resistance to the tendency of the opposing one to become automatic. This is studied with special reference to individual differences. The first of these experiments aims to measure the modifiability of the individual, or the building up of a new association against the opposing and well-fixed former association. The individual differences will be differences in adaptability to a new situation. The second is the same question with which Münsterberg was concerned. The experiment, however, is performed under laboratory conditions, with many subjects, and viewed from the standpoint of individual differences. This second phase nicely supplements the first in a study of interference and adaptability. The paper will also consider the question of the most economical methods of making the two associations automatic. In order to determine more specifically what relation the interference effect holds to the practise effect, the results will be considered in relation to Bergström's contention that the interference effect is equal to the practise effect[5] and Münsterberg's conclusion[6] that the two opposing associations can become automatic.

The question of adaptability is one of great practical importance. Yoakum says: "The biologist tells us that the specialist as a mere individual must fail in the great life functions. The scientists not only demand habits but the power to break those habits. Each and

[5] Bergström, *loc. cit.*, p. 441.

[6] Münsterberg, *loc. cit.*, p. 71.

all abjure over-specialization.''[7] Professor MacDougall says: ''The general character of mental development may be described as adaptation. At all stages and in every phase of its activity the change from the earlier to the later form is a reconstruction which tends to establish more harmonious relation between the individual and his environment. . . . Adaptive reconstruction constitutes the general form of change whether the origin of determination be conceived as lying in the environment and producing adaptive modification, or the element of initiative be considered in the utilization of materials for ideal ends. Such adaptation is incessantly renewed so long as the individual continues to live. . . . Adaptation involves two factors, a form of response already elaborated and an action tending to modify the adjustment in conformity with a variation in the system of stimuli. The former represents the level of adaptation already attained by the organism, the latter represents the increment of advance in which the fact of development consists. The first of these two factors we call Habit, the second Accommodation. Habit constitutes the response of the organism to its environment in so far as the system of stimuli possesses permanence in the course of experience. Accommodation constitutes the organism's response to variations appearing within this system of stimuli.''[8]

Professor MacDougall well describes habit and adaptation in the development of the organism. One of my experiments, that on the typewriter, aims to fulfil under experimental conditions, his outline of habit as a form of response to a system of stimuli already attained, with a change in the environment such that a new level of adaptation is necessary. What I have done in the typewriting experiment before the break is what he calls habit, the development after that break is what he calls accommodation. I am calling it adaptation in a more specific sense.

Adaptation to environmental variations has a very wide significance. Sociologists have long pointed out that the races which can most readily change their customs when thrown into new surroundings are the ones which survive. Le Bon says that one of the fundamental differences between the savage and the civilized races is that of adaptability. Brinton says the same thing and claims that only as there are variations in this respect will the race survive and that the most adaptable are the fittest. Bagehot says that the lowest races

[7] C. L. Yoakum, ''An Experimental Study of Fatigue,'' *Psy. Rev. Mon. Suppl.*, **46**, 116, 1909.

[8] R. MacDougall, ''The System of Habits and the System of Ideas,'' *Psych. Rev.*, **18**, 325, Sept., 1911.

are enclosed in a cake of custom and the first step of progress is to break this preservative habit.[9]

This is also true of smaller groupings of individuals. Giddings[10] makes a classification of religions on the basis of their tendency to break away from hidebound custom. Conversion is a distinct type of changed reaction to life and often presents marked interference. In every condition of life there are interferences of the old situation and adaptability to the new. A strange citizen in a new country, a boy from the farm in the city, the freshman in the college, and the institutional lad on the streets are all familiar examples. A perfect adaptability to all situations is the result of long and varied experience. Adaptations are thus a large part of the activity of any expanding organism.

2. *Investigations on Interference*

The problems of association are among the oldest in psychology, both among the philosophers and among those taking a more experimental attitude toward mental facts. The associationist school attempted to find the unit of their psychology in the discrete ideas and their cohesions, repulsions, and forms of succession. Wundt, Ebbinghaus, Müller, Schumann, Galton, and many others of the pioneers experimented largely with memory, learning, association practise, and other related problems. I shall consider only such investigations as bear directly on some phase of the present problem.

The work that inspired much of later research in interference was an investigation of Münsterberg's published in 1892.[11] He raises the question whether a habit associated with a given sensory stimulus can function automatically while some effect of a previous and different habit association with the same stimulus remains. What is the condition of the old habit when the new one functions automatically? Does it disappear or can the two entirely different movements be connected with the same sensation complex, and either of them be called up independently? He concluded that both can become automatic and need only a slight momentary advantage in order to function. The sensori-motor impulse need not pass out through both pathways of discharge, like an electrical current in-

[9] See Giddings, "Principles of Sociology"; Brinton, "Basis of the Social Mind"; Le Bon, "Psychology of Peoples"; and Bagehot, "Physics and Politics."

[10] F. H. Giddings, "Psychological Classes of Population," *Psych. Review*, Vol. VIII., 337, 1901.

[11] Münsterberg, "Gedächtnisstudien, Beiträge zur Experimentellen Psychologie," **1**, **4**, 70, 1892.

versely proportional to the resistance, but it can go along either one and leave the other undisturbed. Neither is effaced by the repetition of the other but both are retained and can quickly function automatically.

For the investigation of this question he pointed out three experimental conditions: (*a*) The movements must be entirely mechanical so as not to call in the attention; (*b*) they must be easily varied, and (*c*) they must call in the attention whenever a false movement is made or when the reaction is to the previous association. Because of the first requirement the experiments could not be performed in the laboratory.

He therefore performed the experiment upon the simple actions of his daily life. He had been accustomed to carry his watch in his left vest pocket. On the first of the month he put it into his right trousers pocket. He noted the number of false movements and beginnings. On the first of the next month he replaced it in the left vest pocket. During the interval he automatically took it out of the right trouser's pocket. He then found that to relearn taking it out of the left vest pocket took less practise than it previously did to learn to take it from the right trouser's pocket. From this fact he concluded that some traces of the old habit remained. He found that if this alternating process was repeated, the time for each relearning grew less, that both habits constantly grew stronger, and that both became automatic. In fact after the third change there were no wrong reactions.

He also made similar experiments with the inkstands on his writing table, during one period having the right side bottle and during the next the left side bottle filled. He noted the false movements during each period. His third experiment was the alternating use of two doors from his study to the corridor, keeping the one not in use locked, and noting again the number of mistakes until each became automatic.

Müller and Schumann's[12] problem was whether it will require more repetitions to relearn a series of nonsense syllables if in the meantime the syllables of the series have been associated with another set of syllables. They had two series of twelve syllables, each of which was relearned in an average of 7.29 and 7.89 repetitions respectively. The second of the series had been united with twelve new syllables in the meantime. While relearned almost as quickly, interference may have been present, for the second series had been repeated about twice as often as the first, and as considerable part of the work of

[12] Müller and Schumann, *loc. cit.*, 173.

learning the series is in learning the individual syllables, the interference effect was offset by the greater practise effect.

Bergström made several researches on the relation of interference and practise.[13] His first experiment was to study the rate of decrease of interference with the increasing intervals of time between the first and second sorting of cards. His subject sorted a pack of eighty cards into ten piles, eight in a pile, each card of a given pile containing the same picture. In sorting the pack a second time each card might be placed in the same position it occupied before or in any one of nine other positions. If the former happened there would be simple practise, whereas in the latter case the cards enter into associations which would exclude the former associations and there would be interference effect. The subject first sorted the cards, then after a given interval sorted them into ten piles of different positions. The intervals used were 3, 15, 30, 60, 120, 240, 480, and 960 seconds. Bergström found that the average difference between the 3-second and the 8-minute interval was 14.28 seconds, and between the one minute and eight minute interval 4.72 seconds, showing that about two thirds of the decrease took place in the first minute. In twenty-four hours the subject can sort the cards as rapidly as at first. Bergström did not interpret this as meaning that the neural habit of the first association had vanished, but that the second was temporarily raised above it. The time for the first sorting was about 65 seconds and for the one immediately afterwards 85 seconds. From the false movements which were made he concluded that a strong association had been formed.

The second experiment reported about a year later is more elaborate and contains a fuller discussion of the subject of interference.[14] After quoting the problems of Münsterberg and Müller and Schumann he asks what relation the interference effect holds to the practise effect. It must either be equal to, or greater or less than, or hold a variable relation thereto. He used the same number of cards and methods of sorting them as in the previous experiment. The cards for the test proper were sorted in the following manner:

$$A_1^1 A_2^1 A_1^2 A_2^2 \ . \ . \ . \ A_1^8 A_2^8.$$

The comparison series was composed of two sets of cards differing both from A and from each other, and these, with consequently no

[13] John A. Bergström, "Experiments upon Physiological Memory by Means of the Interference of Associations," *Am. Jour. Psych.*, **5**, 356, 1893.

[14] Bergström, *loc. cit.*, 433.

opposing associations, were sorted in like manner eight times. Three minutes for each sorting were given, thus allowing forty-eight minutes for each test, and giving an interval of nearly two minutes between the sortings.

Bergström took eight records for each of the tests, the interference test and the comparison test. For the interference test he used either a different set of cards or allowed several weeks to elapse so that no practise effect from the previous experiment remained. He had one subject and averaged the records for this subject. The comparison test shows a regular practise curve but the A_1A_2 series is a horizontal line. Bergström did not interpret this as indicating that the interfering associations tended to efface one another. This conclusion was verified by the fact that a third arrangement of A showed the same interference effect as either of the other arrangements. He concluded that the interference effect is constant to the practise effect and is in fact equivalent to it.

Bair's experiments were devised to determine the quantitative relation between the increasing permanency of an association and the succeeding practises, and also the increasing amount of interference, first, when there is an alteration in the serial order of stimuli, and second, when there is an alteration of particular responses to particular stimuli. He practised a particular order of stimuli until they became automatic, then changed the serial order of stimuli, or substituted new responses to the old stimuli. The experiments were made on a typewriter with several series or colors arranged in serial order. A certain key was to be associated with each color, and when the color appeared the key was to be struck. The series of colors were then changed or a different set of keys associated with the colors.[15]

He also sorted cards following Bergström's method but did not find nearly as much interference. Taking a lesser number of cards he sorted them into six positions. He had them sorted a varying number of times and then sorted with an opposing arrangement. He found that the difference in time between the last sorting of the first arrangement and the first sorting of the second arrangement increased with the greater number of practises of the first order. He concluded that this greater difference is due to the increased speed of the first arrangement and not to interference.[16]

On the typewriting experiment he sums up as follows: "By practising a particular reaction or series of reactions to a certain stimulus

[15] J. H. Bair, *loc. cit.*, 5, 14.

[16] J. H. Bair, *loc. cit.*, 34.

or series of stimuli, until these responses become automatic, and then associating the same response or series of responses with a different stimulus or series of stimuli, or a different order of responses to the same set of stimuli until the new order becomes automatic, and then returning to the first order, going from one order to the other, every time the order practised becomes automatic, the time becomes continually less for the subsequent adjustments until finally after a sufficient number of alternating adjustment practises, either order can be responded to automatically, one needing but voluntarily to start the response impulse in one direction or the other, and the whole series of responses proceed as though that were the only order acquired.'' His results show that both associations become automatic and that the neural disposition of an old habit does not vanish when a new one is formed. He claimed that there is not as much interference as Bergström finds, and that it is due to indisposition rather than inability.

Mr. W. O. Beazley's problem was to determine the causes of interference and to analyze its elements with the purpose of determining of what it consists.[17] He paid particular attention to interference in relation to motor coordination. In one of his experiments the subject was required to strike five keys in a certain order. The five keys were of the shape and appearance of piano keys, and were constructed in a small box, each key having electrical connections with the kymograph. The first and last keys were connected with a Hipp chronoscope thus giving the time of the entire reaction. Each of the keys had a small picture pasted upon it and the subject had before him a paper on which were the same pictures arranged in the order in which the keys were to be struck. At the given signal the subject struck the keys in the required order, the time was taken by the chronoscope and the objective record was made on the kymograph. Two hundred records were taken, then the arrangement of the three middle keys was changed. The first and last were not altered as they were connected with the chronoscope. With this arrangement two hundred reactions were taken. The second arrangement was then used with a different order for two hundred reactions, and finally the first arrangements was resumed for this second order. Four subjects were used. Practically no interference was found after the changes.

The second part of the experiment was similar to the first except that the two orders were before the subject, and he was instructed

[17] W. O. Beazley, unpublished research work done at the University of Pennsylvania, 1911–12.

to react first to the one, then to the other, alternating through the entire experiment. Very little interference was found. Mr. Beazley concluded that there is no true interference of the old association. The only interference is that due to becoming used to the experiment. These unpublished conclusions are given only as his tentative opinion.

The other works which will be referred to are not directly on the problem but will be used in the discussion, at which time their results will be mentioned.

CHAPTER II

THE TYPEWRITING AND DISCRIMINATION REACTION EXPERIMENTS

The problem as outlined in its broader relations requires a wide field of investigation and observation. The changes in reaction which a community of foreigners undergo on coming into the new conditions of American life, of the American as he goes abroad, or of the country lad going into the city furnish interesting observation. Mathematical problems in which a new element is introduced, a new factor in a problem of logic, a stubborn fact in a theory, a shift in economic conditions, all present situations for a broad study of adaptability. A study of religious conversion presents a distinct type of changed reaction.[1]

The method of the experimental psychologist however is to take processes in as simple a form as he can find them. The aim is, under laboratory conditions, to exclude all factors except the one which is being investigated. Should not the student of individual differences do the same thing? The school of Binet and Henri have contended that the study of individual differences should be concerned with the more intellectual and complex processes because in these the variations are more pronounced.[2] Max Brahn in a criticism of their work says that the problem of individual psychology can only be investigated in the simplest forms of psychical activity.[3] To this latter principle of method the German and American school has generally adhered.

In all the experiments I have used as simple methods as possible in the endeavor to isolate the factor under consideration. Only in an auxiliary study of character by the method of average judgment is the present study concerned with the more general and intellectual differences. This study was made for the purpose of correlation with the results of the typewriting experiment.

(*a*) *Preliminary Group of the Typewriting Experiment*

The first series of experiments were those made on a typewriter and will be referred to as the *typewriting* experiments. There were

[1] See especially E. D. Starbuck, ''Psychology of Religion,'' Chaps. 5 and 13.

[2] Most clearly set forth in ''Etude de Psychologie sur les Auteurs dramatique,'' by A. Binet et J. Passy, *Annee Psychologie, I. S.* 60–118, 1895.

[3] Max Brahn, ''Review of the Work of Binet and Passy,'' *Zeitschrift für Psych. und Phys. der Sinnesorgane,* **12**, 280, 1896.

two experiments, the preliminary, and the regular. In these two the procedure was slightly different. The preliminary experiments were made to familiarize the writer with the subject and to suggest methods for further work.

The preliminary experiments were made during the winter of 1909–10. They were made on an Oliver typewriter, using only the middle row of nine keys. A small stand placed beside the machine held a cover over the keyboard, high enough so as not to hinder the finger or hand movement, yet preventing the subject from seeing the keyboard. This cover also held the copy which was to be written conveniently before the subject. Each key of the row was assigned to a definite finger and was not to be struck by any other finger. The fingers of the left hand manipulated the four keys to the left and those of the right hand the five keys to the right, the index finger manipulating two keys. The experimenter always required that the fingers be in their proper position before the separate repetitions of the copy. The thumb was used for spacing.

For the sake of simplicity numerals instead of letters were written. The keys were numbered from 1 to 9 beginning at the left. The subject could easily remember the number associated with each finger because the numbers came in order from left to right. Each number was associated with a particular finger and that finger was to strike a particular key. The work of the subject was to make the reaction automatic as quickly as possible.

The following three-place numerals were on the copy before the subject and were to be written.

174 479 853 639 751 628 392

The objective record was in letters but these were easily checked up by a key.

The subject's hand and fingers were placed ready to write when the signal was given. The time was taken with a stop watch. It was started on the signal to write and stopped after the last stroke. Thirty seconds rest were given between each trial.

In the preliminary experiment no given number of repetitions were given, as was the case in the later experiments. The purpose was to practise the series until the subjects had reached their maximum speed, and the association had become automatic. This matter will be discussed later.

At this point of practise there was a definite change made in the association of the numerals, and this change will be referred to as the *break*. It was simple but definite. Numbers 2 and 3 which had been

written by the middle and third fingers of the left hand were hereafter to be written by the middle and third fingers of the right hand, and numbers 7 and 8 were to be transposed to their position on the left hand. The number written by the middle finger of one hand was to be written by the middle finger of the other and vice versa, and the same thing is true of the third finger. The copy was not disturbed in the least, only the reaction to the numerals mentioned. The other associations were not disturbed. The change was made to the opposite hand so that the discharge had to follow an entirely different channel, and so that there could be no mistake about *relapses, i. e.,* reacting to the former associations. Had the change been made from one finger to another of the same hand, one could not always tell whether a certain error was due to a bungling of the fingers or to the former association. After the break the subjects again practised until they attained their former efficiency or approximated it as closely as possible.

(*b*) *Regular Group of the Typewriting Experiment*

The procedure in this was the same as in the preliminary experiment except that it was more rigorous and uniform. The same numerals were written under the same conditions up to the break. But the number of repetitions was the same for all the subjects regardless of the point of efficiency reached. Each subject practised the series 130 times. This number of repetitions had been found sufficient for the great majority of the subjects in the preliminary group. The associations seemed well fixed and automatic by the 130th trial. It was also found in the preliminary group that the only safe measure of the strength of the association was the number of repetitions and not the point of efficiency reached. The more rapid subjects could soon attain a speed never reached by others. But to have introduced the break sooner would have made the strength of the associations unequal.

At the end of the 130th repetition the break was introduced. Instead of alternating the fingers writing 2 and 3 with those writing 8 and 7 as in the preliminary experiment, the fingers writing 1 and 3 were alternated with 9 and 7 respectively. This change in the association was explained to them and they were immediately set to writing. Other conditions remained the same as before the break. This new set of responses was then practised fifty times and the experiment closed.

The experiment took six sittings of one half hour or more each. Thirty repetitions were given at each sitting. The first four sittings

gave 120 repetitions. On the fifth day ten repetitions refreshed the habit at which time the break was introduced. Twenty repetitions of the new responses were then given on that day and thirty on the day following. The experiments were all held in the afternoon or early evening and at approximately the same hour each day of the experiment.

Simple errors in writing before the break were counted, grouped, and studied for their own interest. But the errors after the break in which the old associations persist were separately treated and were the direct material for the study of individual differences. These will be referred to as *relapses* and are always to be distinguished from errors.

(c) *First Group of the Discrimination Reaction Experiments*

The results reported in this group are only a by-product of the experiments on the influence of caffein on the various mental functions held under the direction of Dr. Hollingworth in the early months of 1911.[4] The writer was one of the assistants in those experiments and secured permission to use these results for a study of individual differences in interference. The data here given are those secured during the first week before caffein was used.

The experiments are those of color discrimination reactions on a Forbes chronoscope. The subjects reacted to "red" with the right hand and to "blue" with the left. Only reactions of the right hand are recorded, while the left set a buzzer going. Mistakes were noted as well as double reactions. The data are for 15 subjects, 10 men and 5 women. There were 140 reactions for each hand taken in fourteen sittings of ten reactions each for each hand. At the end of this practise period the associations were reversed, blue now being reacted to with the right hand and red with the left. The time and errors of the right hand were recorded. This association was then practised for three periods of ten reactions each. Interference is measured by the difference in reaction time of discrimination due to the change. The greater frequency of errors after the change is also a measure of interference.

Later on when the subjects had become well practised on the Color Naming Test, the Opposites test and the Calculation test, Dr. Hollingworth had the correct responses for each of these tests written out, and ascertained the time it took the subjects to merely read the answers to the tests. The average time was taken as approximately

[4] H. L. Hollingworth, "The Influence of Caffein," ARCHIVES OF PSYCHOLOGY, **22**, April, 1912.

the time required for the perception and pronunciation of the words. This average measure of the perception and pronunciation time was taken from the average records of the subjects for the test proper. This is perhaps as fair a measure of the individual's ability in these subjects as can be obtained, for it reduces the time to the ''psychological limit'' if there is such a thing. The results obtained on the chronoscope experiments were then correlated with these various tests along association lines.

The chronoscope experiments were conducted by Margaret Hart Strong, of the department of psychology of Barnard College, under the direction of Dr. Hollingworth.

(*d*) *Second Group of the Discrimination Reaction Experiments*

This series is similar to the first group except that the reaction times for both red and blue were recorded and that only sixty records of each were obtained before the color associations were changed. After the change thirty reactions with each hand were taken. Before the change the right hand reacted to ''red'' and the left to ''blue''; after the change the reactions were reversed. The group was composed of twelve men and seven women, all of them subjects in the typewriting experiments. The data were secured for the purposes of correlation with the results of the typewriting experiments.

(*e*) *Character Judgments*

This is the only part of the investigation which is not of an experimental nature, and which deals with the more general and intellectual qualities. Determination of character by the method of average judgments is most fully discussed by Professor Norsworthy.[5] This is the method here used.

An attempt was made to secure competent judgments on the following qualities or traits of character: mental balance, intellect, emotions, will, quickness, originality, individuality, independence, and persistency of habits. Only three of these qualities are discussed with reference to our problem, viz., independence, originality, and individuality. Sheets were prepared giving the names of the subjects in a horizontal column at the top and a vertical column giving the qualities in which they were to be rated. These sheets were sent to twelve men who were well acquainted with all the subjects to be rated. Judgments were secured upon the eight men of the preliminary type-

[5] Naomi Norsworthy, ''Judgments of Character,'' Volume to William James, 551, 1908.

writing experiment. Their rating in these qualities was correlated with the results of the typewriting experiments.

The observers were instructed to rate each of the subjects in the given traits on the basis of 10. The individual being the most fully developed in the trait was to be rated 10 and the lowest conceivable was to be rated 1. The most important consideration was to secure the same standard of judgment. For this purpose it was felt better to make the highest individual conceivable the basis of 10 rather than the highest in the group. The writer realizes at the present that greater individual differences would have been secured had 10 and 1 been made the highest and lowest of the group rather than the given standards.

A letter of instruction was given to each of the observers, in which the comparative rating of the men was emphasized. Whether a man is rated higher or lower is not as significant as whether he is in the correct relative position. Only ten judgments were secured, as two of the observers did not rate all the men. The judgments for a single trait were averaged for each individual and his place in the group determined by that average. The method of position in the group could not be used because two or three subjects might have the same marking, which would introduce too great an error.

(*f*) *Subjects*

The subjects in the typewriting experiments range in age from 20 to 35. Only one of them had special training in psychology. None of them had had appreciable practise on a typewriter.

The designations for the different subjects are as follows:

In the preliminary group: Hf, Co, Sa, Lo, Ja, Se, Hi, St, all students of Union Theological Seminary.

In the regular group: Men, Ha, McC, Br, De, La, Cl, students at Union Theological Seminary. Ru, clerk in the seminary.

Women: Mrs. Hf and McC, wives of students in Union Seminary, Wh, instructor in psychology in Teachers College. Mo, Gr, Wa, Ta, Og, and Ha, students at Teachers College.

The subjects of the first group of discrimination experiments will not be referred to individually. Those of the second group are all included in the typewriting groups. In order to facilitate matters for the reader all men will be referred to by the designations given while the designations of women will be prefixed by Mrs. or Miss. The context or definite statement will indicate in what experiment. There were twenty-four subjects in the typewriting experiments.

II. Statement of Results

To make the treatment more coherent a number of questions will be discussed in Chap. IV., on "Individual Differences," which will embody direct statement of results. Such results as will not be needed to make this chapter clear will be given there.

(a) *The Typewriting Experiments*

Table I. gives in condensed form the results of both the preliminary and regular groups. The individual records of the 130 repeti-

TABLE I

Table I. gives first trial before and after the "Break" in the first two columns; the average of the first five likewise in the second two columns; then the average of last five. The next two columns give the absolute gain and following the percentage of gain before and after "Break." The last column gives the difference between the last five before the "Break" and the last five after it. A negative sign indicates poorer efficiency after "Break" than before, positive better record than before "Break."

	First Trial		Average of First Five		Last Five		Absolute Gain		Percentage Of Gain		
Subject	Before	After	Before	After	Before	After	Before	After	Before	After	
Ha	36.0	48.4	28.8	29.4	6.6	9.8	22.2	19.6	.77	.66	—3.2
Mo	75.0	36.0	49.0	26.0	8.8	9.6	40.2	16.4	.82	.63	— .8
Ta	54.0	37.4	39.4	29.4	10.0	10.2	29.4	19.2	.76	.61	— .2
Hf	35.2	19.6	28.8	20.0	11.0	11.0	27.8	9.0	.62	.45	.0
McC ...	96.0	39.0	44.6	32.6	10.6	10.4	34.0	22.2	.75	.69	.2
Wa ...	69.8	28.0	41.4	26.6	9.8	15.0	31.6	11.6	.76	.45	—5.2
Wh ...	22.4	27.8	20.8	18.4	6.6	8.0	14.2	10.4	.69	.56	—1.4
Og	54.0	32.2	34.8	24.6	6.8	12.2	28.0	12.4	.79	.51	—5.4
Gr	57.0	41.0	44.6	29.4	12.4	12.2	32.2	17.2	.72	.58	.2
Cl	33.2	24.2	24.4	18.8	6.6	9.4	17.8	9.4	.73	.51	—2.8
Ru	77.4	57.0	47.0	35.0	16.8	13.8	30.2	21.2	.64	.60	3.0
Ha	131.0	32.6	58.0	26.6	12.8	14.8	45.2	11.8	.78	.45	—2.0
Br	55.6	37.0	41.8	25.6	10.6	14.6	36.2	11.0	.77	.44	—4.0
De	37.0	33.0	28.0	28.0	9.8	10.4	28.2	17.6	.65	.63	— .6
La	47.0	28.0	33.4	25.8	11.2	13.0	22.2	12.8	.70	.50	—1.8
McC ...	71.8	35.4	52.4	31.8	14.0	20.0	38.4	11.8	.74	.38	—6.0
Hf	46.2	31.0	47.0	26.8	17.0	17.2	30.0	9.6	.64	.36	— .2
Co	51.0	72.0	38.6	42.8	13.0	11.6	25.6	31.2	.67	.73	1.4
Sa	49.2	29.6	35.8	21.8	11.6	12.0	24.2	9.8	.68	.46	— .4
Lo	60.0	36.0	35.0	30.6	11.6	11.2	23.4	19.4	.67	.63	.4
Ja	40.0	44.0	32.0	35.8	9.4	11.6	22.6	24.2	.74	.68	2.2
Se	45.8	31.0	30.2	21.6	10.4	10.0	19.8	11.6	.65	.55	.4
Hi	41.0	24.2	28.4	17.6	9.0	12.4	19.4	15.2	.68	.31	—3.4
St	28.0	25.0	23.6	20.4	9.6	9.8	14.0	10.6	.60	.51	— .2

tions are not given as they constitute a simple practise curve, fairly regular and reaching, for most subjects, the physiological limit. The table gives the times of the first performance, the average of the first five and last five performances, the absolute gain in seconds and the percentage of gain. All these are given both before and after the break. Plate I. gives the curves for the men and women of the regular group. The curves of the preliminary group are not given. Both the curves and the table show the practise before the break and its effect. The break, it will be recalled, was transferring 1 and 3 from the fingers of the left hand to the respective fingers of the right hand, and vice versa.

The initial records of the practise curve are very high because the subjects were not familiar with the typewriter, and were very awkward at first. They improved rapidly because of the simplicity of the experiment. That the subjects do not rise as high after the break as in the initial performance of the practise curve is due to the great gain made in learning to use the machine. Four subjects, Co, Ja, Misses Ha and Wh, do actually rise higher after the break than at the beginning. The curves clearly show that in the fifty repetitions after the break the subjects did not attain the level of efficiency before the break. An extra column is therefore given in Table I. showing the difference between their final performances in the two curves. This is obtained by subtracting the average of the last five performances after the break from the average of the last five before the break. A negative sign indicates that they did not reach their practise level by the given number of seconds and a positive sign indicates that they surpassed it. By this column we find that among the women McC, Hf, and Gr, equalled or surpassed their record before the break, while the rest did not equal it. Among the men of the preliminary group Se, Lo and Co, and among those of the regular group, Ru surpassed their previous record, while the rest did not equal it. Miss Wa and Mr. McC show the largest difference, they being six seconds or more behind their former record. The rate of improvement will be dealt with in Chap. IV.

Perhaps the best index of interference of the old association and of its persistency is that of its recurrences. Whenever 1 or 3, and 7 or 9 were struck with the fingers with which they had been associated, the errors were treated as a recurrence of the former association, and as such were used as an index of interference. These recurrences are called *relapses.* Table II. shows the distribution of the relapses in periods of ten trials and the total number for each subject.

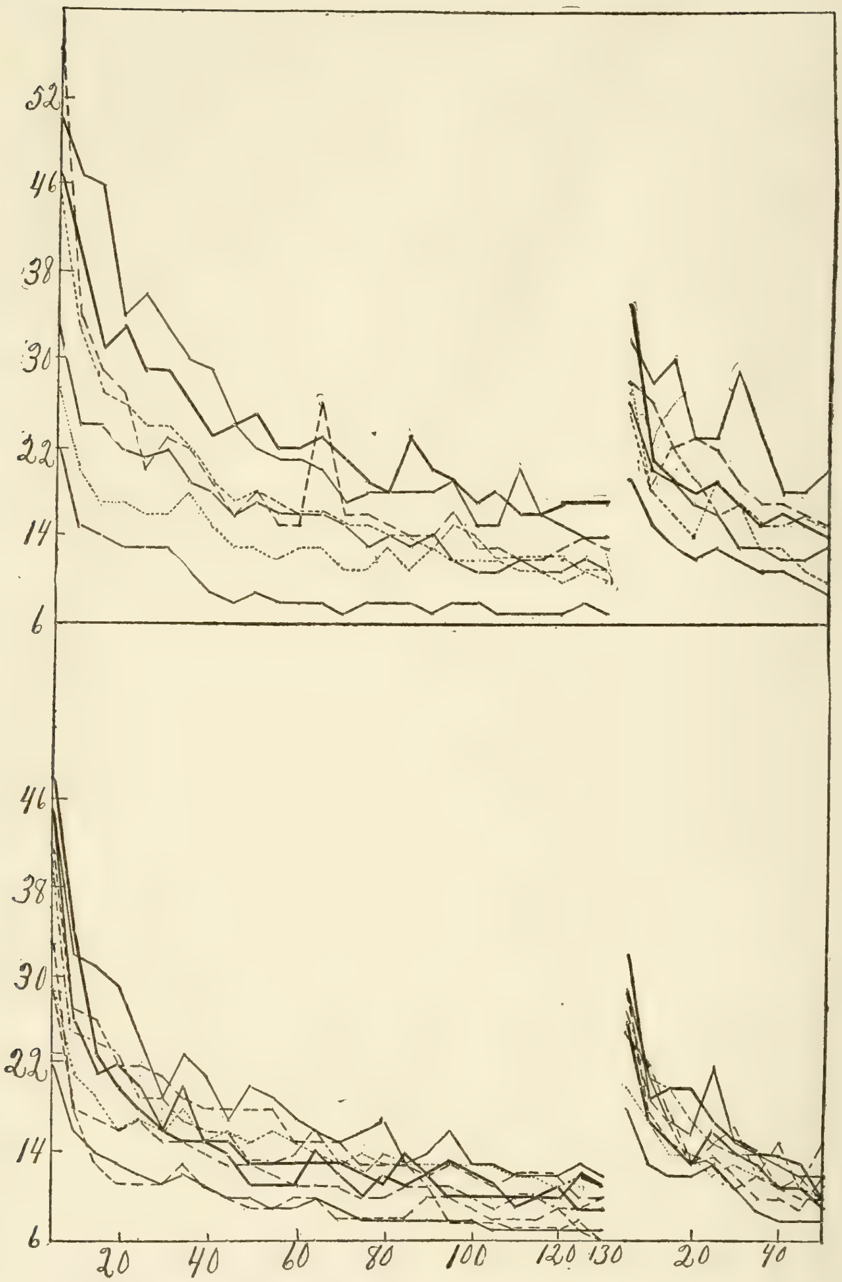

Plate I. gives the records of the regular group in the type-writing experiment. The ordinates give time in seconds and abscissas the number of repetition. The curves are made out on the basis of average of five reactions. The individual variations are due largely to confusion, errors, etc., and are not important for

TABLE II

The distribution of relapses is shown for every ten repetitions after the break. Two of the subjects of the preliminary group had more than fifty repetitions after the break, thus giving a wider distribution of their errors.

	1–10	10–20	20–30	30–40	40–50	50–60	60–70	Total
Women								
Ha	5				1			6
Mo	12	1	1	1				15
Ta	8	2	1					11
Hf	6	2	4					12
McC	11	25	16	11	3			66
Wa	16	2	13	16	12			59
Wh	1				1			2
Og	39	3	6	3	3			54
Gr	15	14	24		3			56
Men								
Cl	13		2	1				16
Ru	15	2	3					20
Ha	16	9	10	3	1			39
Br	8	12	5	3	5			33
De	5	7	2	1				15
La	2	4	2	3	7			18
McC	3	2	14	10	13			42
Ja	3	2	4	2	1			12
Se	7	1	4					12
Hf	22	7	6	5	9	24	7	80
Hi	5	6	8	1	3			23
Co	1	1			1			3
St	6	4						10
Sa	5	1	2		1	2		11
Lo	10	4	1	1				16

With the exception of Hf of the preliminary group who continued two periods longer, which accounts for his eightly relapses, the women show the greater variability. Mrs. McC has 66 and Miss Wh has 2; among the men the extremes are De 15, McC 42. Among the men of the preliminary group the number of relapses ranges from 3 for Co to 80 for Hf. Hf however made 31 of those in the repetitions beyond fifty which was the total number of repetitions for all of subjects except Hf and Sa. With the exception of Hf the distribu-

our purpose, and to give them would require many charts or cloud the general curve. After 130 repetitions the break is shown and the record for the 50 repetitions after the break with the variations in performance. While some of the curves have similar lines they are well enough separated so as not to be confused.

tion in the preliminary group is very regular, 3, 10, 11. 12, 12, 16 and 23. Among the men of the regular experiment there are two groups one of 15, 16, 18, and 20, and the other of 33, 39, 42. Among the women there are also two groups, the first 2, 6, 11, 12, 15, then a wide gap to 54, 56, 59, 66. That this distinct separation into groups should exist does not seem to the writer to justify any other statement than that there is a very wide range of individual differences. There is no correlation between the rate of improvement and the number of relapses so that those making the greater number of relapses did not do so in a sacrifice to speed. The differences in the number of relapses are far greater than the differences in rate of improvement or of speed before the break.

There were many expressions of difficulty after the break, and these were noticeable among the four women making the most relapses. One quivered and gritted her teeth, another started to cry, the two others claimed they tried hard and one said that it felt so funny to start to put one finger down and then stop and put the other down. These were the four who made 54, 56, 59 and 66 relapses. The number of subjects does not warrant one in concluding that there are two types among the women, yet the results do tend to show that there is not a single type to which women conform when put under such a situation of adaptation to a new element. While the men arrange themselves in groups the differences are not so great.

The men in the preliminary group with the exception of Hf conform closely to a type. Hf, whose time record was poor and who thought he had no associations firmly fixed, nevertheless shows an abundance of relapses after the break.

A glance at the table will show interesting differences as to the stage at which the relapses occur. The first twenty trials were on the day of the break and the other thirty on the day following. With most of the subjects the majority of the relapses were made during the first ten trials but with some this was not the case. Ha, Mo, Ta, and Og, among the women, and Cl, Ru, Se, St, Sa, and Lo among the men, had a majority of their relapses during the first ten trials and thereafter a rapid decrease. But this is not true of Mrs. McC, Misses Wa and Gr, nor of Ha, Br, De, McC, Ha, Ja, and Hi among the men. They had more relapses during the later periods. Mrs. McC made more relapses in each of the following three periods than in the first one. McC's relapses rise very decidedly on the second day though he had a good record on the first. On the third day Hf shows more errors in the first period than on either of the two previous days. On the second day he had apparently acquired good con-

trol, but on the third day he had 6, 3, 1, 4, 4, 4, and 2 relapses per trial in consecutive trials. Although he thought he had no habit formed it proved to have greater and more persistent effect on him than on any other individual of the entire group. Miss Gr had nearly as many relapses during the first ten trials of the second day as on the twenty trials immediately after the break. The comparative ease with which some of the subjects rose to the situation immediately after the break, and the relapses to which they were subject later on was very noticeable. This is especially true of Hf and McC, Misses Wa and Gr, and Mrs. McC. On the other hand Miss Og had 39 relapses during the first ten trials, then a very rapid falling off. These pronounced differences show that with some the effect is more immediate than with the others. The subjects were always cautioned on the second day to give their attention to the work so as not to make any relapses. In some cases the attention was not so well sustained as immediately after the break. Hi who made more relapses on the first period of the second day than before admitted wavering attention. But as a general rule those who made the most errors seemed to try the hardest. This was especially true among the women. On the other hand St, who was very rapid and had no relapses on the second day had much wavering of attention. De was more attentive, less nervous, and had better control after the break than before.

The only three-place number not having a digit affected by the break was 628. The 2 and the 8 were however transferred several times. The 8 was transferred from the third finger of the right hand to the third finger of the left and *vice versa* with the 2. This was done because of the third finger being between the two fingers whose associations were transferred. Miss Wa who did this several times, said that it seems she must also transpose the other finger (pointing to the third finger) because the middle and fourth fingers had to change. The same subject also had a number of cases in which she struck with the correct finger and then with the other as before the break. This seems to be due to a sort of secondary impulse after the correct finger has responded. There were also a number of cases, especially Misses Ta and Mo, where the relapse was first made, then the correct association.

The correlations by the Pearson coefficient between the rate of improvement after the break and the relapses are as follows:

	Women	$r = -.09$	P.E. .28
Regular	Men	$r = -.76$	P.E. .10
Preliminary	Men	$r = -.69$	P.E. .12

The correlations between the rate of improvement after the break and before are as follows:

Regular group	Men	$r = .13$	P.E. .19
	Women	$r = .45$	P.E. .17
Preliminary group	Men	$r = -.86$	P.E. .06

These coefficients show a negative correlation between the rate of improvement and relapses among the men. Those with more relapses improve less rapidly and those with fewer relapses improve more rapidly. Among the women there is no correlation. This shows that speed was not purchased at the price of accuracy nor accuracy secured with unlimited time. The two elements of errors and time are difficult to adjust so as to secure a reliable measure. With unlimited time the subject would have made no relapses, with a given speed there would be very many. How can we measure the difference when both time and relapses are variable?

The fact is that in order to secure a measure of the ability of breaking the former association the speed could not be set for the various subjects, for with some the physiological limit of writing is so much lower than with others, that they could at a given speed have time to inhibit each tendency to relapse, while the others going at their normal speed would have many relapses. The effect of the association can be measured only when each subject is writing at his best possible speed. Nor can a standard of absolutely no relapses be set, for then each single reaction could be fully thought out and the new association correctly made. The only true measure is to have the subject write at his greatest possible speed and measure both time and relapses.

The negative correlation shows however that speed is not made at a sacrifice of accuracy, nor accuracy purchased at the expense of speed. This shows that the two elements do not annul each other as an index of interference, but rather emphasize the differences. The individual differences are greater than either the relapses show or than the rates of improvement show and to the extent to which the two are negatively correlated is the variability of the group increased. Again there is no correlation between the rate of improvement before and after the break except that of .45 P.E., .17 among the women. This shows that the rapidity of improvement after the break is not due simply to the general ability to improve, but that the differences are entirely due to interference. There is thus no relation whatever between the performance after the break and before, and the individual differences after the break are due to interference. The negative correlation between the relapses and the rate of improve-

ment shows that individual differences in adaptability to a new situation in the face of the persisting old association are greater than under the conditions of ordinary practise. And the negative or negligible correlations between the rate of improvement after and before the break shows that the differences are not due solely to the same qualities as make for efficiency in practise. These differences are further emphasized by the fact that the variability in the rate of improvement after the break, as expressed by the Pearson coefficient of variability, is 1.6 times as great as that before the break among the women, 2.11 times as great among the regular group of men, and 4.3 as great for the preliminary men.

(*b*) *Second Group of the Discrimination Reaction Experiments*

These are reported here because they were given directly for purposes of correlation with the results of the typewriting experiment. In this group there were given sixty reactions to each of red and blue to the right and left hand respectively. Then the order was changed and the colors associated with the opposite hand. The results of the twelve men and seven women are given in Table

TABLE III

Table III. gives in the first column of each hand the average of the thirty reactions previous to the change; in the second column the average of the thirty after the change. The third column gives the difference between the two. A negative sign indicates that the time was less after the change than before. All numbers represent sigmas.

	Right Hand			*Left Hand*		
	Before Red	After Blue	Difference	Before Blue	After Red	Difference
Miss Wh	305	333	28	332	312	—20
Wa	345	347	2	371	375	4
Ta	375	387	12	392	380	—12
Gr	398	376	—22	372	337	—35
Mrs. McC	443	494	51	488	400	—88
Ha	362	398	36	403	435	32
Mo	431	444	13	461	456	— 5
La	349	303	—46	353	298	—55
McC	476	478	2	477	476	— 1
Br	391	388	— 3	391	429	38
Ha	371	392	21	401	443	42
Ru	276	292	16	281	310	29
Cl	336	385	49	383	367	—16
De	316	343	27	349	331	—18
Hf	367	388	21	379	392	13
Lo	226	233	7	231	191	—40
St	351	353	2	332	343	11
Hi	308	293	—15	309	291	—18
Co	283	370	93	337	348	11

III. The results are indefinite because of an insufficient number of associations before the change was made. The subjects had not reached their practise limit and consequently interference is clouded and in some cases the reactions are quicker after the changes than before.

A glance at the table will show that nearly half of the records after the change are better than those before. The only explanation seems to be that the practise limit had not been reached. The results are negative and there is no correlation with the typewriting experiment. I give the table in order to show all the experiments, those which show negative as well as positive results.

(c) *Character Judgments*

Table IV. gives the rating of the eight men in the preliminary group of the typewriting experiment in the traits of *individuality, originality* and *independence.* These figures give the averages of ten ratings by friends who lived in the same dormitory with the subjects and associated with them in the class room. All the observers were graduate students. The observers were very conscientious in their work and the ratings seem as carefully prepared as any that could be secured. With the exception of Se in the trait *independence* no P.E. is over 1, and the median P.E. is .8. This is of course quite large considering the small individual differences. The tendency among most observers is to mark the individuals high and make very little difference between them, giving many of them the same rank.

TABLE IV

The average ratings of the character judgments with their probable errors are given under the appropriate headings. The fourth column gives the number of relapses in the typewriting experiment. The fifth column gives the number of mistakes in pronouncing ''the'' as ''a'' in a selection of prose of 300 words in which the word ''the'' occurred forty times. The subjects were required to read as rapidly as possible and pronounce every ''the'' as if it were ''a.''

	Individuality		Originality		Independence		Typewriting	Reading
	Av.	P.E.	Av.	P.E.	Av.	P.E.	Relapses	Mistakes
Se ...	6.0	1.0	7.2	.8	7.0	2.0	9	6
Lo ..	6.0	1.0	5.5	.5	6.0	1.0	12	10
Hi ..	8.5	.5	8.0	.5	9.0	1.0	27	7
Co ..	6.5	.5	6.0	.0	7.5	.5	5	8
Ja ...	8.0	.0	7.0	1.0	7.5	.5	13	8
Hf ..	5.5	.5	5.0	1.0	5.8	.2	80	17
St ...	9.0	.5	9.2	.8	9.0	1.0	13	6
Sa ..	6.5	.5	6.0	.8	8.0	1.0	11	19
Av. ..	7.0	1.0	6.6	.6	7.5	.5	22 P.E. 10	10 P.E. 4

The correlations of the rating in these traits with the results of the typewriting and reading experiments are given below. Since the number of errors and relapses is an inverse index of adaptability, the correlation will have an opposite sign from the relation which the two actually sustain to each other. Few mistakes and relapses must go with high rating and *vice versa* in order that there should be a positive correlation. The correlations are all negative in sign but are therefore given positive to show the real correlation of the abilities in breaking the old association.

Between Individuality and "Relapses," Pearson Coeff.	=.40	P.E. .20
Between Originality and "Relapses," Pearson Coeff.	=.35	P.E. .21
Between Independence and "Relapses," Pearson Coeff.	=.50	P.E. .18
Between Individuality and Reading Mistakes, Pearson Coeff.	=.66	P.E. .13
Between Originality and Reading Mistakes, Pearson Coeff.	=.64	P.E. .14
Between Independence and Reading Mistakes, Pearson Coeff.	=.43	P.E. .19
Between "Relapses" and Reading Mistakes, Pearson Coeff.	=.53	P.E. .17

I was unable to secure competent observers for the subjects in the other group, because no sufficient number of observers could be secured who were acquainted with all the subjects.

(*d*) *First Group of the Discrimination Reaction Experiments*

Table IX. embodies the results of the first group of the discrimination reaction experiments. These results are much more definite than those of the second group. The old habit was much better established, there being 140 reactions to red before the change to blue was made. In the second group there were only 60. Column *D* gives the interference, *i. e.*, the difference in sigmas between the average of the last thirty reactions to red, and the average of the thirty to blue after the change. This difference varies among the individuals from 2 sigmas to 107. Column *F* gives the percentage of this interference to the subject's practised reaction. Column *E* gives the number of errors due to interference of the old habit, calculated from the difference between the number of errors in the thirty reactions before and after the change.

The other columns give in seconds the "psychological limit" for the tests specified, the time for perception and pronunciation of the name being subtracted. There were four women and eight men in this experiment.

The correlation between *D* and *F* by the Pearson coefficient is .85 P.E., .05. We thus find that the individuals showing the greater interference in discrimination time also show greater interference in errors. This same thing was found true of the men in the type-

writing experiments, while among the women there was no correlation.

There is a slight correlation among some of the association tests and column *D*. Between *A* and *D* the correlation by the Pearson coefficient is .46 P.E., 15, by the method of unlike signs, $r = .70$. There is practically no correlation between *D* and either *B* or *C*.

TABLE IX

Table IX. gives the results of the first group of chronoscope discrimination experiments obtained under the direction of Dr. Hollingworth. The twelve subjects are numbered as reported in the caffein experiment. Nos. 3, 6, 11, 15 are women, the rest are men. Column *D* gives the difference in sigmas of the average of 30 reactions of the right hand to red, and the average of 30 reactions to blue, the subjects being long practised to red and then changed to blue. Column *E* gives the percentage of difference on the basis of their discrimination reaction time before the change. Column *F* is obtained by subtracting the wrong reactions in 30 trials before the change with the wrong reactions in 30 trials after the change, thus giving the greater number of errors under interference. Columns *A, B,* and *C,* respectively, give the difference in seconds required to take the color-naming test (100 colors), the opposites test, and the calculation test, less the time of pronouncing the answers to each of the tests when written out. It thus gives the time taken for each test, excluding individual differences in rapidity of enunciation. The individual records are the average of ten trials for each test.

	A	*B*	*C*	*D*	*E*	*F*
Subjects	Color-Naming Test Difference between Naming and Articulation	Opposite Test Difference between Associating and Naming	Calculation Difference between Calculating and Naming	Interference in Discrimination on Chronoscope	Per Cent. of Interference	Errors
1	31.8	19.3	63.2	7	.022	1
3	24.6	13.4	59.0	79	.125	8
4	14.7	9.5	57.4	32	.120	3
6	11.2	4.2	30.6	32	.100	0
7	9.6	8.9	32.2	2	.006	1
8	21.5	20.7	44.6	14	.038	0
9	14.4	13.1	45.8	52	.164	4
10	19.7	19.3	37.8	26	.104	5
11	23.5	17.2	52.7	38	.122	3
13	34.7	16.0	41.7	107	.384	11
14	20.6	10.3	46.0	35	.102	4
15	42.5	28.7	100.0	56	.124	2
Av.	22.4	15.0	50.9	40	.118	3.5

There does seem to be a clear cut correlation between the performance in the color naming test and the interference in the chronoscope discrimination. The naming of colors has an element of inter-

ference, because the impression of the one color persists during the perception and attempted naming of the second, and the same ability that gives efficiency in changing easily from one reaction to another would give a high efficiency in naming one-hundred colors on the color chart.

The correlation between *D* and *F* shows that here as elsewhere those having greater interference in time also have greater interference in errors and those having less in the one have less in the other.

CHAPTER III

The Card-sorting Experiment

I. *Description of the Experiment*

This experiment was made during the winter and spring of 1911 and the fall and winter of 1912. There were 34 subjects, 17 men and 17 women. The experiment took six periods of about 35 minutes each for each subject.

The experiment is an enlargement and modification of the card sorting experiment which Bergström used.[1] Instead of using cards with pictures, which are likely to be of different degrees of complexity and difficulty, and of different degrees of familiarity with different subjects, or words, which in themselves might have interfering associations, I used "flinch" cards. These are very smooth and are easily shuffled, and have simple numbers, and the numbers are printed at both ends so that they are always right side up. The cards are 2 by $3\frac{1}{2}$ inches in size. The numbers used were from 1 to 10, and hence are of equal familiarity and difficulty. There were eighty cards used, eight of each kind from 1 to 10.

The cards were not to be sorted at random, as in Bergström's experiment, but had to be thrown into boxes made for them. Fig. 1 shows the boxes and the arrangement for two sortings. These boxes made an ideal arrangement for easy sorting. The boxes have no bottom so that after each sorting the frame is lifted up, put into another position, and is ready for a second sorting. The little boxes are four inches square and hence are plenty large enough to receive the cards. The back of each box is 2 inches higher than the front so that the cards must fall in the right boxes unless wrongly thrown. There is no possibility of a card slipping from one section to another and a card on the wrong pile means that it was thrown wrongly. The subjects were required to pick up cards which they dropped and to pick them out of the box if wrongly thrown. Thus there are no errors to take into consideration.

The time was taken with the stop watch. The watch was started when the signal was given to the subject and stopped when the last card was thrown. The cards were always thoroughly mixed, special care being taken that two of the same kind did not follow each other. There were enough packs of cards and enough helpers to

[1] Bergström, *loc. cit.*, pp. 434, 435.

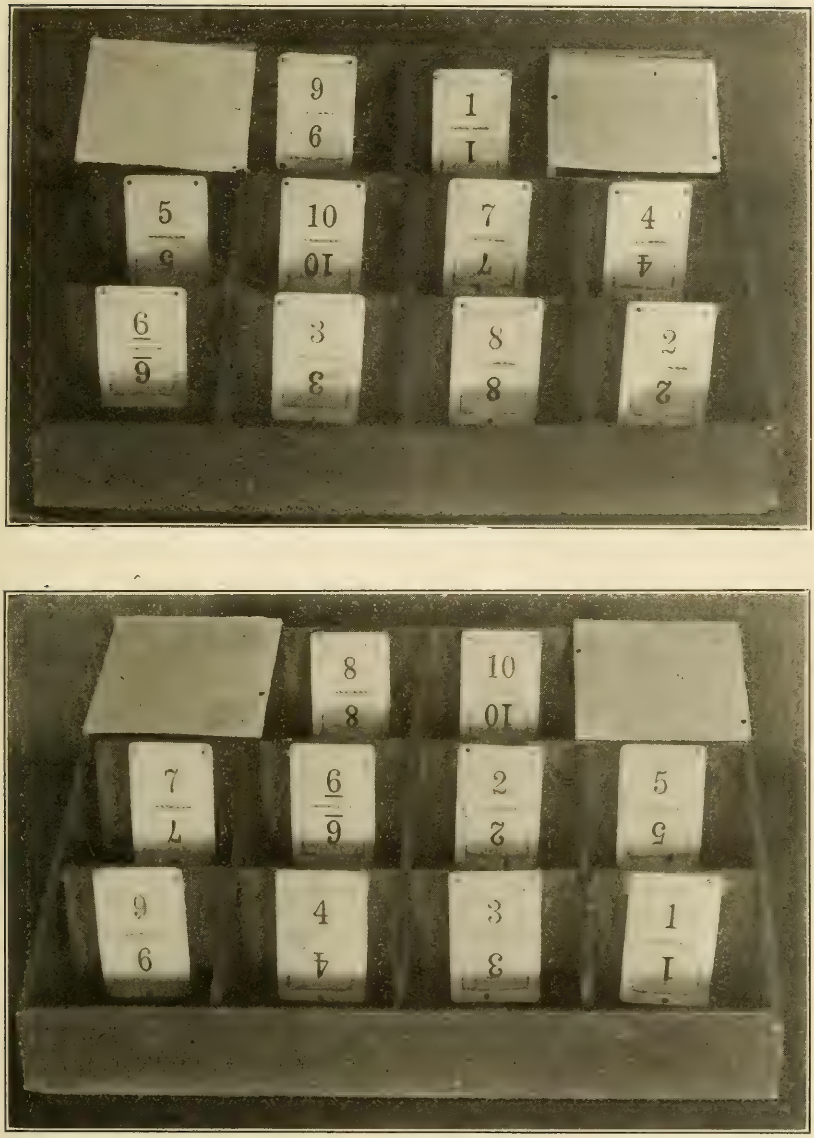

FIG. 1. Description in text.

keep the subject going no matter how rapid he was. A rest of 30 seconds was always given between sortings whether of the same or opposing arrangements. This was kept constant for Bergström had shown that the interference decreases rapidly with the increase of the interval between the sortings. Sixteen sortings were taken at

one sitting which continued for six days giving a total of 96 sortings, 48 of each arrangement. The element of fatigue did not enter in at all, as at no time were more than sixteen sortings held.

The subjects were divided into four groups, an equal number of men and women in each group. The men and women for each group were chosen at random. The first group is composed of four men and four women and is known as the "practise group" or Group I. This group sorted the cards only into the one arrangement and are used to determine the practise curve without interference as a comparison test for the other three groups. They sorted the cards 48 times.

The second group was composed of three men and three women. They sorted alternately according to the following formula:

$$A_1^1, A_2^1, A_1^2, A_2^2, A_1^3, A_2^3, \ldots, A_1^{48}, A_2^{48}.$$

(A_1 throughout the discussion will indicate the first arrangement, and A_2 the second, while the exponential numbers will give the number of times the respective arrangements are sorted.) Each arrangement was thus sorted eight times each day for six days, alternating with the other arrangement. Bergström had the alternating arrangement, but the conditions here are different from those in Bergström's experiments in that the interval between the sortings is thirty seconds, while his were nearly two minutes; in that each arrangement is fixed for all the subjects and kept constant; and in that it is continued six times as long. This group will be referred to as the alternating group or Group II.

The third group was under the same conditions as Group II., except that the first arrangement was sorted four times, thus being fixed more firmly than in Group II., then the other arrangement four times, and then to the first again according to the following formula:

$$A_1^4, A_2^4, A_1^4, A_2^4, \ldots, A_1^{48}, A_2^{48}.$$

On the same day there were only two periods of each arrangement, which made sixteen sortings in all. The first arrangement for the first section was not subject to interference, but the second arrangement always was and the second section of the first was also. This group had seven men and seven women and will be referred to as Group III.

The fourth group was like the third except that the cards were sorted into the first arrangement eight times, and then eight times into the second according to the following formula:

$$A_1^8, A_2^8, A_1^8, A_2^8, \ldots, A_1^{48}, A_2^{48}.$$

In this group the first arrangement was never subject to immediate previous interference, as only one turn at each arrangement could be had at a sitting. The second arrangement had the interference effect of eight previous sortings of the first, but was itself continued for eight, giving it greater time to overcome the interference. This group, which will be referred to as Group IV., had three men and three women.

The experiments were held in the afternoon or early evening on successive days. In several cases a day had to be missed, but no influence was detected. Where however the subject had to remain away for considerable time, he had to be dropped. Several subjects had to be thrown out because of such irregularity.

The subjects ranged in age from 16 to 36. Only two of them had special training in psychology. They will be referred to by the following designations.

Group I. Men: Wa, Co, Ol, and Be, all students at Union Theological Seminary. Women: Sm and Co, clerks; Fo and St, students.

Group II. Men: Em, clerk; Gr, theological student; My, graduate student in psychology. Women: Cu and Mt, married women with high school training; Bk, musician.

Group III. Men: Ca, Si and Dr, theological students; Wi and Rd, high school students; Sv, architectural draughtsman, Sp, salesman. Women: Ku, Wo, Sv, Su, Ar, wives with high school and college training; Br and Th, clerks with high school training.

Group IV. Men: As and Bl, theological students; Cu, graduate student in psychology. Women: Mrs. Th, with high school training, Mor and Ag, music teachers.

All men will be referred to by the initials and women will be referred to by initials prefixed with Mrs. or Miss. The context or definite statement will show in what group the subject under consideration performed.

II. *Results of the Experiments*

As stated in the description of the experiments Group I. will be used as a practise group for comparison and the other three groups in a study of interference. Group II. sorted the cards alternately, Group III. sorted them four times by one arrangement and then four times by the other, and Group IV. sorted them first eight times the one way, then eight times the other.

The individual records of Group III. are given in Table V. This group had seven men and seven women and is the only one that will be used for a study of individual differences. The other groups had

three men and three women each and have their value principally in the discussion of the relative interference effect of the various methods, and the rapidity with which the associations can become automatic in each.

The curves for this group are given in Plate II. for the women and III. for the men. Plate IV. shows the average curves for the men and women of this group, together with the performance in the practise group for purposes of comparison. The Roman type of the table gives the records of the first arrangement and the Italic type those of the second.

Table VI. gives the averages and their deviations for the four groups. Plate V. gives the graphic record of the averages for Groups II. and IV. The Roman and Italic types give the records of the first and second arrangements respectively. Since the first group had only 48 sortings and those of only one arrangement they are all printed in Roman numerals and are continued through the

Plates II. and III. give the curves for the women and men, respectively, in the card sorting experiment, Group III., Arrangement A_1 four times, A_2 four times. Each plate gives seven curves, four in the lower sections and three in the upper. The ordinates give the time in seconds and the abscissas the number of times sorted. The days are marked by the lines with the numerals, each day having sixteen performances, and the change from one arrangement to another is shown by the smaller graduated scale. The procedure each day is made clear by the arrangement being shown on the third day. The rise in time after each change is clearly seen in both, though much more pronounced with the men. The curves in the lower sections are allowed to extend into the upper at the beginning to show them in full.

To avoid overcrowding the plate the following references to curves are given: Plate II. Women: Lower section, heavy line, Mrs. Wo; heavy dotted, Miss Br; light, Ku; light dotted, Sv. Upper section, heavy, Mrs. Ar; light, Mrs. Su; light dotted, Miss Th.

Plate III. Men: Lower section, heavy, Wi; heavy dotted, Sv; light, Sp; light dotted, Rd. Upper section, light, Ca; heavy, Dr; light dotted, Si.

Plate IV. gives the average curves in the card sorting experiment, Group III., for the 7 men and 7 women. The average practise curve for the men and women is also given. Solid line curve gives records of women, dotted of men. The change in arrangement is so clearly seen in the spires of the curves that no marking is necessary. Every day has its three rises after the three changes. It is also noticeable that each day's beginning is about at practise level of previous day. The curves of the men will be seen to rise higher after the change than the women, although their average is lower until the fourth day, when they begin to remain as low or lower.

Plate V. gives the curves for men and women in the second and fourth groups, Group IV. above, Group II. below. Solid line, women; dotted line, men. The scale on the ordinate is just half that of the average curve of Group III.

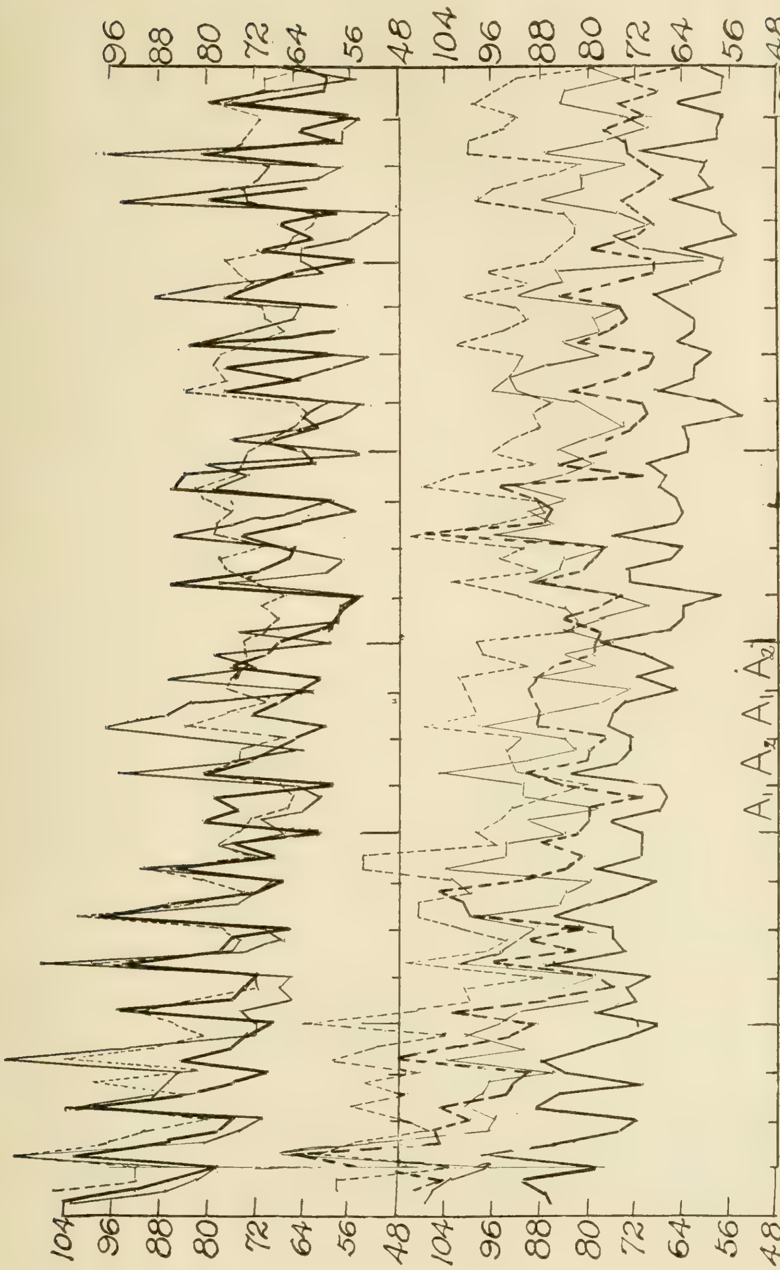
104 96 88 80 72 64 56 48
104 96 88 80 72 64 56 48
16 32 48 64 80 96
A1 A2 A1 A2

PLATE II

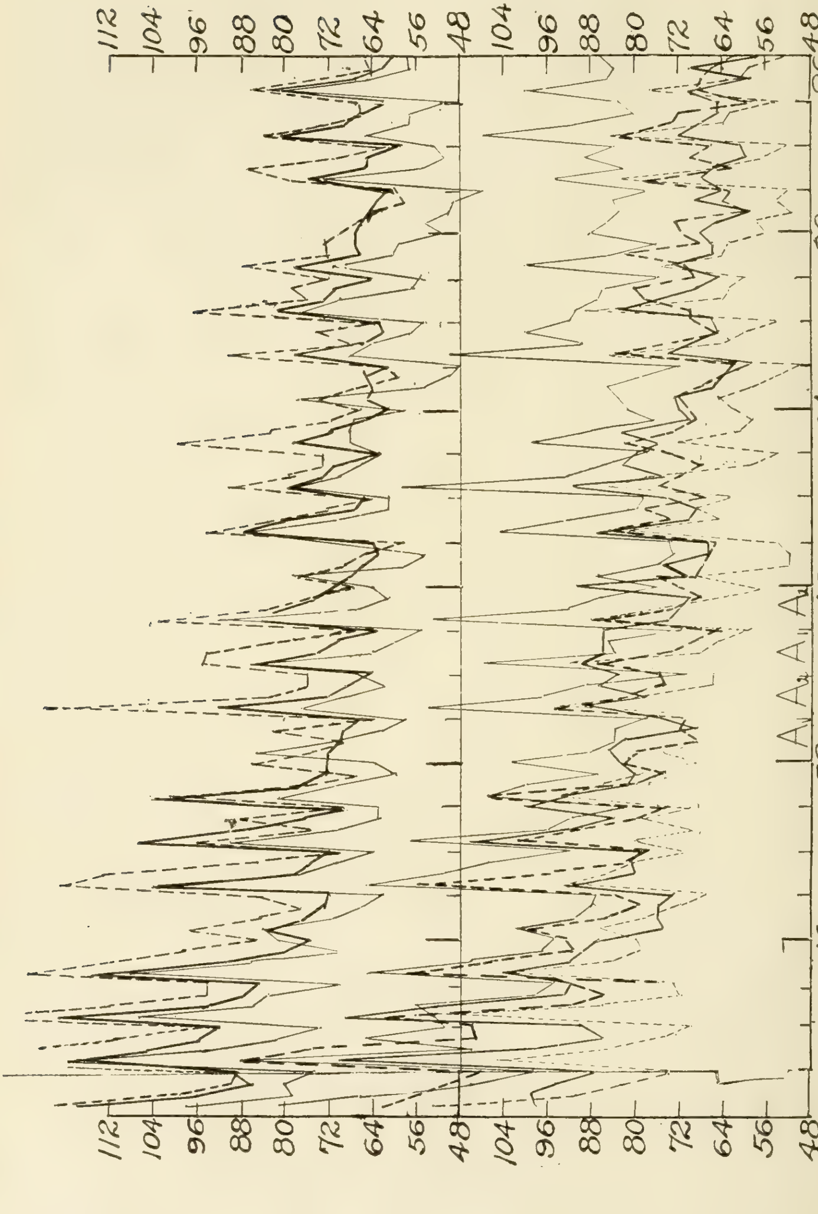

Plate III

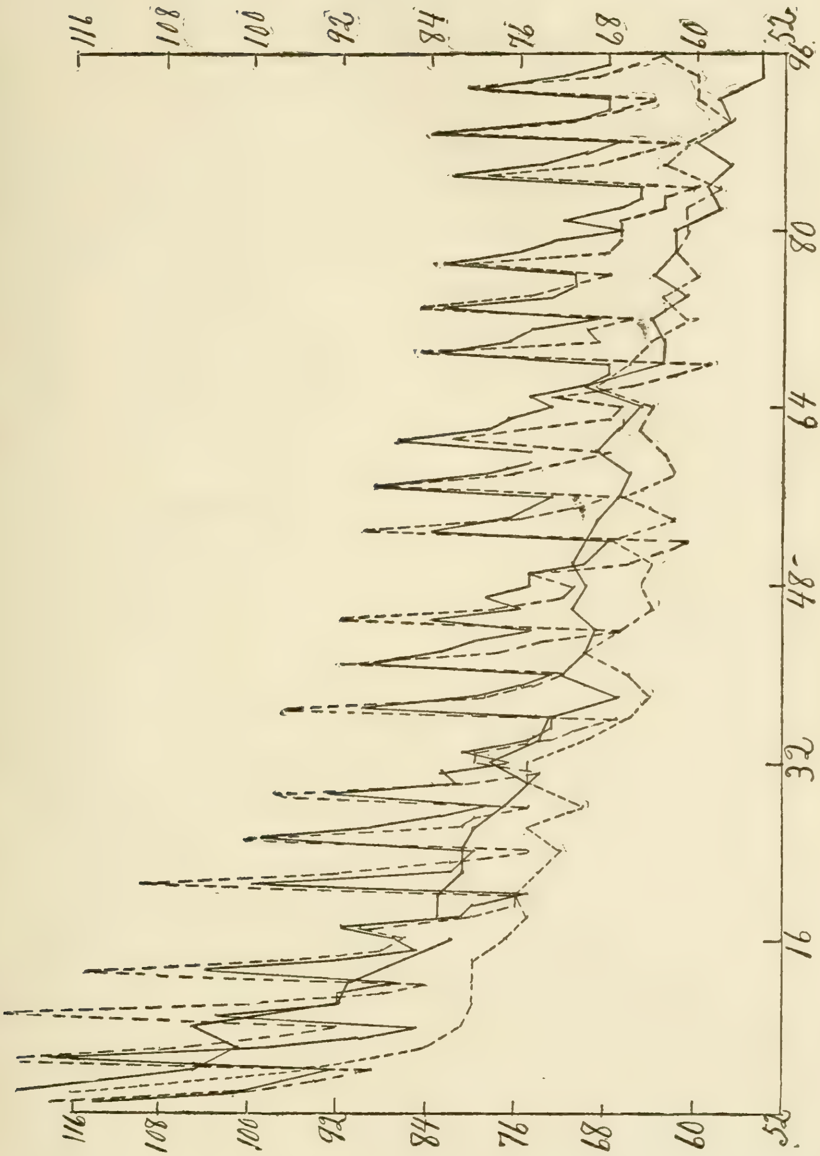

PLATE IV

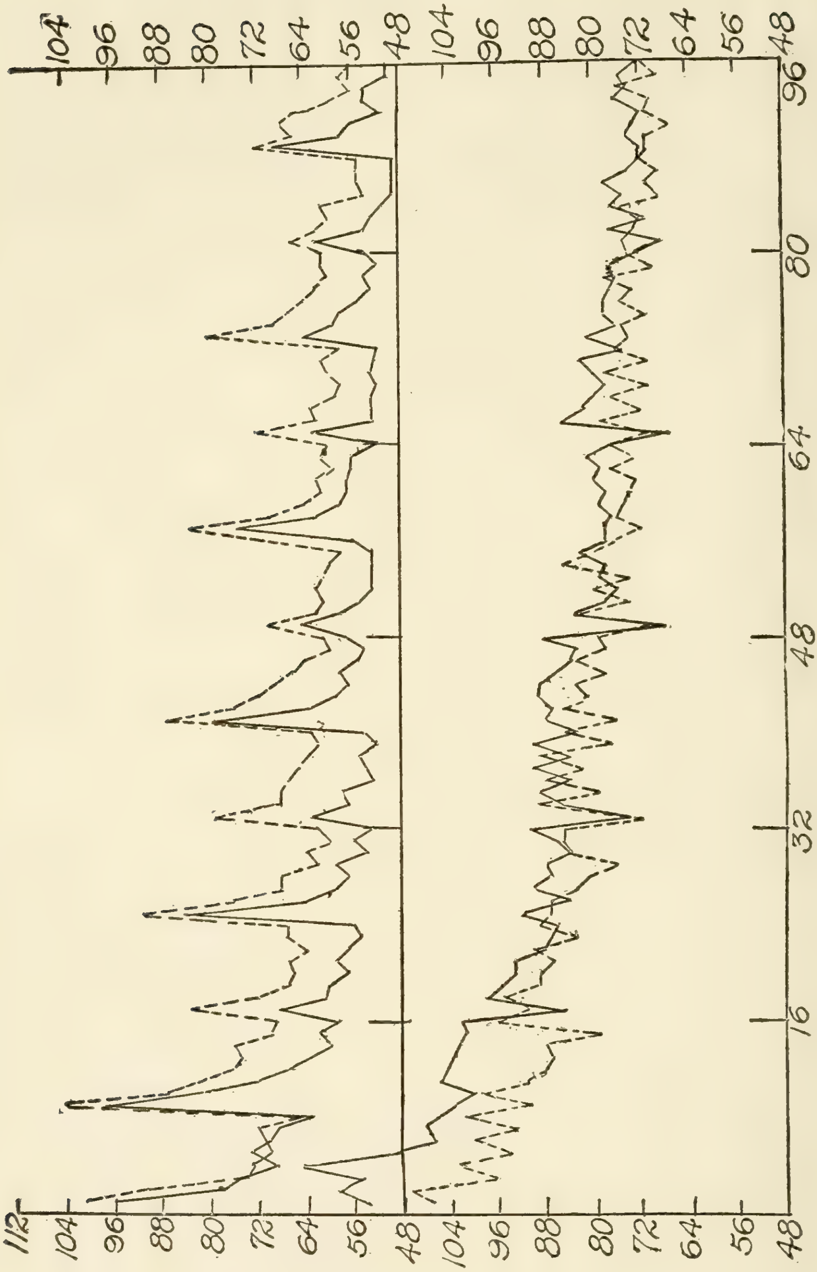

PLATE V

TABLE V

Individual Records of Women in Card-sorting, Group III

The Roman type gives records in seconds of Arrangement 1; Italic type, of Arrangement 2. Spaces separate successive days.

	Ku	Sv	Wo	Br	Su	Th	Ar	Average	A.D.
1	107	138	86	140	103	118	104	113.7	15.8
2	106	122	87	109	87	106	92	101.3	10.9
3	103	122	91	104	82	92	85	97.0	10.9
4	97	104	79	118	79	92	79	92.6	11.4
5	*132*	*126*	*98*	*130*	*113*	*121*	*103*	*117.6*	*11.0*
6	*119*	*120*	*86*	*105*	*81*	*97*	*87*	*99.4*	*13.1*
7	*97*	*107*	*75*	*106*	*75*	*91*	*79*	*89.7*	*11.4*
8	*96*	*112*	*72*	*100*	*71*	*80*	*76*	*85.2*	*13.8*
9	101	120	89	105	104	100	100	102.6	5.9
10	98	111	85	94	89	84	82	91.6	7.1
11	97	118	71	93	87	100	76	91.7	11.7
12	86	109	84	90	85	82	70	86.6	7.1
13	*104*	*123*	*88*	*112*	*114*	*104*	*85*	*104.3*	*10.3*
14	*91*	*116*	*80*	*102*	*93*	*89*	*76*	*92.4*	*9.6*
15	*101*	*104*	*73*	*93*	*72*	*81*	*73*	*85.2*	*12.2*
16	*95*	*128*	*68*	*89*	*72*	*86*	*69*	*86.7*	*14.4*
17	92	109	79	103	75	92	95	92.1	8.7
18	80	100	72	87	66	83	76	80.6	8.0
19	85	101	74	85	68	72	74	80.1	9.0
20	79	88	70	79	66	73	72	75.3	5.7
21	*102*	*111*	*87*	*96*	*108*	*100*	*94*	*99.7*	*6.3*
22	*95*	*97*	*74*	*82*	*74*	*76*	*78*	*82.1*	*8.0*
23	*92*	*94*	*76*	*90*	*67*	*75*	*76*	*81.4*	*9.0*
24	*89*	*102*	*76*	*81*	*71*	*78*	*66*	*80.4*	*8.7*
25	100	109	86	100	98	102	100	99.3	4.1
26	85	109	80	101	75	85	87	88.9	9.2
27	84	100	73	105	73	74	73	83.1	11.3
28	80	103	69	91	69	81	67	79.4	9.4
29	*104*	*118*	*79*	*84*	*91*	*90*	*86*	*93.1*	*10.1*
30	*94*	*118*	*71*	*81*	*70*	*73*	*69*	*82.1*	*13.6*
31	*94*	*95*	*71*	*88*	*78*	*78*	*76*	*82.9*	*8.1*
32	*84*	*99*	*71*	*82*	*67*	*75*	*61*	*77.0*	*9.7*
33	90	95	76	80	70	73	81	81.0	6.6
34	78	93	68	80	64	67	75	75.0	7.4
35	91	86	67	71	61	66	79	74.4	7.4
36	91	80	68	85	65	68	59	73.7	10.3
37	*105*	*92*	*83*	*91*	*95*	*81*	*80*	*89.6*	*7.0*
38	*90*	*93*	*75*	*80*	*76*	*75*	*71*	*80.0*	*6.7*
39	*82*	*94*	*71*	*80*	*64*	*75*	*65*	*75.9*	*8.1*
40	*84*	*91*	*71*	*77*	*60*	*67*	*60*	*72.6*	*9.4*
41	98	108	77	89	97	84	73	89.4	10.0

TABLE V (*continued*)

	Ku	Sv	Wo	Br	Su	Th	Ar	Average	A.D.
42	87	99	76	88	87	77	70	83.4	7.7
43	78	100	74	89	83	70	64	80.0	7.9
44	73	101	65	90	62	77	61	75.3	12.0
45	*89*	*102*	*72*	*89*	*87*	*76*	*76*	*84.4*	*8.4*
46	*79*	*90*	*66*	*86*	*72*	*73*	*70*	*76.3*	*7.4*
47	*84*	*98*	*71*	*79*	*79*	*74*	*66*	*78.7*	*7.6*
48	*74*	*99*	*78*	*79*	*59*	*74*	*61*	*75.0*	*9.0*
49	84	83	66	78	75	70	66	74.6	5.3
50	78	82	64	84	58	68	58	70.3	7.6
51	70	84	64	78	58	71	57	68.7	8.0
52	77	92	58	75	55	67	54	68.3	11.4
53	*90*	*103*	*73*	*89*	*78*	*72*	*86*	*84.4*	*8.6*
54	*85*	*89*	*73*	*81*	*60*	*77*	*71*	*76.6*	*7.3*
55	*84*	*95*	*66*	*79*	*57*	*78*	*66*	*75.0*	*10.0*
56	*77*	*91*	*64*	*77*	*67*	*71*	*65*	*73.1*	*7.3*
57	96	110	76	103	86	79	74	89.3	11.7
58	86	95	65	87	76	79	64	78.8	9.1
59	90	87	64	86	67	76	55	75.0	11.1
60	84	89	65	89	59	81	60	75.3	12.0
61	*92*	*108*	*68*	*95*	*79*	*82*	*86*	*87.1*	*9.6*
62	*84*	*102*	*67*	*71*	*73*	*75*	*84*	*79.3*	*9.1*
63	*79*	*89*	*70*	*85*	*80*	*74*	*62*	*77.0*	*7.1*
64	*85*	*96*	*63*	*77*	*55*	*73*	*64*	*73.3*	*10.9*
65	80	93	63	77	76	69	70	75.4	8.6
66	74	88	64	73	64	65	61	69.9	7.3
67	78	89	54	70	57	63	64	67.9	9.6
68	82	86	59	71	54	65	60	68.1	10.0
69	*96*	*92*	*69*	*83*	*77*	*84*	*77*	*82.7*	*7.1*
70	*93*	*93*	*62*	*75*	*66*	*77*	*64*	*76.6*	*9.6*
71	*87*	*92*	*62*	*70*	*59*	*79*	*77*	*75.4*	*9.4*
72	*78*	*89*	*59*	*69*	*53*	*78*	*58*	*69.1*	*10.9*
73	84	102	65	82	83	82	81	82.7	6.4
74	78	95	62	77	74	67	58	73.0	9.1
75	79	90	62	73	65	70	59	71.3	8.0
76	75	92	65	75	64	71	58	71.4	8.0
77	*92*	*101*	*69*	*85*	*89*	*76*	*77*	*83.4*	*8.1*
78	*84*	*90*	*63*	*76*	*79*	*72*	*71*	*76.3*	*6.9*
79	*85*	*97*	*58*	*69*	*60*	*75*	*65*	*73.1*	*11.3*
80	*60*	*87*	*57*	*69*	*64*	*77*	*55*	*67.0*	*9.1*
81	71	85	64	80	64	67	71	71.7	6.3
82	76	82	55	72	56	65	62	66.9	8.4
83	70	82	57	69	52	62	68	65.1	7.3
84	75	84	58	72	49	61	58	65.3	10.0
85	*88*	*99*	*66*	*74*	*94*	*73*	*80*	*82.0*	*9.8*
86	*81*	*96*	*59*	*68*	*76*	*74*	*63*	*73.7*	*9.1*
87	*81*	*87*	*61*	*67*	*61*	*71*	*61*	*69.9*	*8.4*

TABLE V (*continued*)

	Ku	Sv	Wo	Br	Su	Th	Ar	Average	A.D.
88	*73*	*81*	*60*	*70*	*57*	*69*	*61*	*67.4*	*6.8*
89	87	100	71	73	97	77	81	83.7	9.4
90	77	100	58	74	57	74	58	70.7	11.1
91	70	94	58	77	57	72	64	67.7	9.3
92	74	92	57	71	54	71	56	67.9	10.4
93	*85*	*99*	*65*	*75*	*80*	*76*	*77*	*79.6*	*7.4*
94	*84*	*96*	*57*	*68*	*69*	*70*	*60*	*72.0*	*10.3*
95	*74*	*92*	*57*	*73*	*54*	*70*	*59*	*68.4*	*11.4*
96	*80*	*78*	*61*	*64*	*64*	*64*	*65*	*68.0*	*6.3*

TABLE V

INDIVIDUAL RECORDS OF MEN IN CARD-SORTING, GROUP III

The Roman type gives records in seconds of Arrangement 1; Italic type, of Arrangement 2. Spaces separate successive days.

	Ca	Si	Dr	Sv	Wi	Sp	Rd	Average	A.D.
1	98	122	118	117	98	145	127	117.9	11.7
2	79	99	96	99	91	120	120	100.6	10.9
3	80	90	86	83	95	110	115	94.1	10.7
4	75	89	89	74	88	99	109	89.0	8.6
5	*120*	*146*	*118*	*105*	*134*	*152*	*152*	*132.0*	*15.4*
6	*95*	*127*	*105*	*85*	*98*	*110*	*139*	*107.7*	*14.4*
7	*82*	*111*	*96*	*75*	*86*	*129*	*109*	*98.3*	*15.4*
8	*74*	*94*	*92*	*70*	*90*	*115*	*110*	*92.1*	*9.6*
9	107	135	121	116	133	117	125	122.0	7.7
10	86	112	94	86	116	120	92	100.1	13.0
11	83	94	88	72	93	98	86	88.0	6.0
12	70	94	85	73	92	95	76	83.0	9.0
13	*108*	*127*	*115*	*103*	*104*	*129*	*122*	*115.4*	*9.0*
14	*86*	*112*	*91*	*84*	*92*	*110*	*105*	*97.1*	*10.1*
15	*70*	*98*	*80*	*79*	*90*	*97*	*91*	*88.1*	*10.1*
16	*81*	*85*	*75*	*80*	*85*	*95*	*93*	*85.6*	*4.4*
17	84	97	83	86	75	102	100	89.6	8.5
18	71	84	78	78	76	91	85	80.3	5.1
19	66	77	73	72	76	88	80	76.0	4.6
20	62	84	72	67	73	87	84	75.7	8.1
21	*103*	*121*	*104*	*92*	*93*	*129*	*120*	*110.2*	*13.6*
22	*77*	*113*	*78*	*85*	*80*	*115*	*95*	*91.9*	*13.4*
23	*71*	*85*	*76*	*80*	*81*	*107*	*82*	*81.7*	*5.3*
24	*64*	*70*	*72*	*71*	*79*	*92*	*77*	*75.0*	*6.4*
25	90	96	107	80	110	121	102	100.6	11.1
26	70	75	87	70	90	96	90	81.4	7.6
27	63	88	76	79	84	92	80	80.3	6.4
28	63	69	69	70	100	82	74	75.3	7.3
29	*81*	*101*	*104*	*94*	*93*	*107*	*106*	*98.0*	*7.3*
30	*71*	*76*	*78*	*86*	*81*	*100*	*80*	*80.7*	*7.5*
31	*60*	*67*	*73*	*79*	*80*	*87*	*75*	*74.4*	*6.5*
32	*63*	*86*	*72*	*69*	*83*	*103*	*82*	*79.7*	*10.0*

TABLE V (*continued*)

	Ca	Si	Dr	Sv	Wi	Sp	Rd	Average	A.D.
33	85	75	72	75	85	88	80	80.0	5.1
34	71	69	70	67	82	82	69	73.1	5.2
35	62	82	69	61	69	82	72	71.0	6.4
36	58	64	66	58	76	78	71	67.3	6.4
37	*77*	*124*	*92*	*94*	*89*	*118*	*95*	*96.7*	*15.4*
38	*69*	*83*	*72*	*79*	*78*	*97*	*81*	*79.4*	*5.5*
39	*62*	*76*	*69*	*66*	*87*	*89*	*75*	*74.9*	*7.6*
40	*67*	*76*	*64*	*66*	*83*	*71*	*76*	*71.9*	*5.4*
41	86	95	85	92	90	108	87	91.9	6.0
42	64	94	74	68	86	84	73	77.7	9.0
43	60	82	70	65	86	85	69	73.9	9.0
44	55	67	63	59	86	72	65	66.7	7.0
45	*92*	*103*	*84*	*88*	*73*	*117*	*88*	*92.1*	*7.2*
46	*67*	*83*	*82*	*76*	*72*	*92*	*73*	*77.9*	*6.5*
47	*61*	*75*	*75*	*65*	*70*	*87*	*68*	*71.9*	*6.1*
48	*64*	*68*	*68*	*58*	*91*	*76*	*72*	*71.0*	*7.1*
49	77	78	68	69	71	87	76	75.1	5.0
50	58	69	66	52	75	76	68	66.3	6.4
51	54	65	63	52	67	73	67	63.0	5.5
52	48	58	64	54	67	74	65	61.4	6.6
53	*87*	*94*	*91*	*81*	*87*	*105*	*85*	*90.0*	*5.7*
54	*70*	*83*	*80*	*65*	*70*	*93*	*74*	*76.1*	*7.7*
55	*61*	*72*	*67*	*69*	*69*	*80*	*78*	*71.1*	*5.3*
56	*61*	*64*	*65*	*63*	*74*	*79*	*67*	*67.6*	*5.3*
57	78	90	79	85	91	123	76	88.9	10.7
58	70	79	73	78	75	93	72	77.1	5.3
59	66	73	71	59	82	86	68	72.1	7.3
60	63	73	63	54	80	78	73	68.3	6.4
61	*68*	*99*	*78*	*72*	*76*	*99*	*82*	*82.0*	*9.7*
62	*68*	*82*	*71*	*61*	*83*	*88*	*73*	*75.1*	*7.9*
63	*67*	*71*	*65*	*59*	*70*	*77*	*69*	*68.3*	*3.9*
64	*58*	*66*	*61*	*62*	*72*	*81*	*70*	*67.1*	*6.1*
65	77	74	65	65	73	83	72	72.7	4.6
66	54	67	64	59	66	85	68	66.1	6.1
67	50	59	65	55	64	79	64	62.3	6.6
68	48	62	61	50	59	72	62	59.1	5.9
69	*68*	*90*	*78*	*85*	*74*	*114*	*84*	*84.7*	*10.0*
70	*64*	*68*	*65*	*64*	*69*	*90*	*66*	*69.4*	*6.3*
71	*57*	*74*	*62*	*60*	*65*	*100*	*70*	*69.7*	*10.0*
72	*54*	*64*	*63*	*54*	*66*	*92*	*70*	*66.1*	*8.4*
73	77	97	81	89	83	91	78	85.0	6.3
74	70	76	73	64	75	85	80	74.7	4.9
75	57	79	71	63	69	86	69	71.0	5.4
76	55	72	64	60	65	76	71	67.6	7.4
77	*71*	*88*	*78*	*75*	*73*	*100*	*82*	*81.3*	*8.0*
78	*60*	*72*	*66*	*64*	*66*	*85*	*66*	*68.4*	*5.6*
79	*59*	*73*	*67*	*59*	*66*	*76*	*70*	*67.1*	*5.0*
80	*51*	*70*	*67*	*56*	*68*	*88*	*72*	*67.4*	*8.0*

TABLE V (*continued*)

	Ca	Si	Dr	Sv	Wi	Sp	Rd	Average	A.D.
81	54	67	66	58	65	85	73	67.0	7.1
82	50	64	62	51	59	83	60	61.1	7.0
83	49	58	63	54	69	84	64	62.9	8.1
84	44	60	61	52	64	78	63	60.3	7.0
85	*73*	*79*	*76*	*83*	*68*	*95*	*78*	*79.3*	*5.4*
86	*55*	*87*	*65*	*66*	*69*	*82*	*62*	*69.4*	*8.9*
87	*51*	*71*	*65*	*56*	*60*	*89*	*70*	*64.6*	*7.7*
88	*53*	*61*	*59*	*52*	*61*	*84*	*66*	*62.0*	*6.9*
89	65	84	81	85	72	108	83	82.6	8.6
90	57	74	69	64	68	91	73	70.9	7.3
91	57	66	66	63	65	80	72	67.0	5.1
92	51	67	62	54	65	83	60	63.6	6.6
93	*81*	*86*	*83*	*77*	*70*	*100*	*69*	*80.9*	*7.7*
94	*65*	*78*	*67*	*61*	*61*	*85*	*68*	*69.3*	*6.9*
95	*57*	*64*	*62*	*59*	*70*	*84*	*66*	*66.3*	*6.0*
96	*57*	*65*	*60*	*53*	*58*	*86*	*62*	*63.0*	*7.1*

TABLE VI

Table VI. gives averages and A.D.'s of all the groups of Card-Sorting Experiment. Group I., being the Practise Group and having only one arrangement, has only 48 records, but are given throughout whole length of experiment for purposes of comparison. Roman type gives Arrangement 1; Italic type, Arrangement 2.

	Group I				*Group II*				*Group III*				*Group IV*			
	Men		Women		Men		Women		Men		Women		Men		Women	
1	116.3	17.3	121.3	15.8	107.0	7.3	118.6	16.0	117.9	11.7	113.7	15.8	100.7	11.7	96.3	15.0
2					*111.3*	*8.3*	*124.3*	*17.0*	100.6	10.9	101.3	10.9	91.0	4.7	78.0	12.7
3	95.0	6.0	105.0	13.5	95.7	6.0	119.3	9.3	94.1	10.7	97.0	10.9	73.7	4.7	74.7	9.0
4					*101.7*	*11.7*	*129.0*	*13.3*	89.0	8.6	92.6	11.4	73.3	6.7	69.3	6.7
5	84.3	7.8	101.0	10.5	94.3	11.0	111.7	9.7	*132.0*	*15.4*	*117.6*	*11.0*	70.3	3.3	73.0	10.7
6					*100.3*	*6.3*	*107.0*	*3.3*	*107.7*	*14.4*	*99.4*	*13.1*	71.0	8.0	70.0	6.7
7	81.3	5.8	104.5	14.5	92.7	7.0	108.0	5.0	*98.3*	*15.4*	*89.7*	*11.4*	72.3	10.7	69.0	4.7
8					*101.3*	*1.7*	*105.7*	*2.3*	*92.1*	*9.6*	*85.2*	*13.8*	64.7	3.0	62.7	5.0
9	80.3	7.3	91.7	12.3	96.0	7.7	102.7	3.0	122.0	7.7	102.6	5.9	*104.0*	*2.0*	*97.0*	*4.0*
10					*99.0*	*6.7*	*99.7*	*1.0*	101.1	13.0	91.6	7.1	*87.3*	*11.0*	*80.7*	*4.0*
11	79.7	6.3	90.3	8.3	91.3	7.7	104.7	4.3	88.0	6.0	91.7	11.7	*82.3*	*11.0*	*71.7*	*6.3*
12					*88.0*	*8.0*	*104.3*	*5.0*	83.6	9.0	86.6	7.1	*76.0*	*10.0*	*66.7*	*3.7*
13	80.0	7.0	85.7	9.3	87.3	9.0	103.0	6.0	*115.4*	*9.0*	*104.3*	*10.3*	*75.3*	*13.7*	*64.0*	*4.0*
14					*88.3*	*9.0*	*102.0*	*4.7*	*97.1*	*10.1*	*92.4*	*9.6*	*76.0*	*12.0*	*60.3*	*5.0*
15	77.3	5.8	81.5	10.3	79.0	8.0	100.7	5.7	*88.1*	*10.1*	*85.2*	*12.2*	*70.0*	*11.3*	*62.0*	*4.7*
16					*96.0*	*10.0*	*103.3*	*3.7*	*85.6*	*4.4*	*86.7*	*14.4*	*68.7*	*8.0*	*59.3*	*3.7*
17	75.3	7.3	83.3	9.3	90.7	12.3	84.7	7.0	89.6	8.5	92.1	8.7	83.0	14.7	69.3	3.7
18					*95.3*	*1.7*	*98.0*	*5.3*	80.3	5.1	80.6	8.0	71.4	14.0	60.7	4.3
19	76.3	4.3	83.0	10.0	89.0	8.7	95.3	1.7	76.0	4.6	81.1	9.0	66.7	10.7	59.7	3.0
20					*88.7*	*7.0*	*92.7*	*5.0*	75.7	8.1	75.3	5.7	66.0	10.0	56.7	2.3
21	74.3	4.8	81.3	11.8	87.0	10.0	92.7	5.7	*110.2*	*13.6*	*99.7*	*6.3*	66.4	11.3	58.7	3.7
22					*90.0*	*10.7*	*87.7*	*3.7*	*91.9*	*13.4*	*82.1*	*8.0*	64.0	10.3	56.7	3.0
23	72.0	2.5	80.5	7.0	83.0	6.7	87.0	4.0	*81.7*	*5.3*	*81.4*	*9.0*	67.3	5.3	55.3	4.3
24					*88.7*	*1.7*	*86.0*	*2.7*	*75.0*	*6.4*	*80.4*	*8.7*	66.7	8.3	56.0	1.7

TABLE VI (*continued*)

	Group I				*Group II*				*Group III*				*Group IV*			
	Men		Women		Men		Women		Men		Women		Men		Women	
25	74.5	5.5	80.3	12.8	86.7	5.7	92.0	2.3	100.6	11.1	99.3	4.1	*91.0*	*10.7*	*84.3*	*8.3*
26					*87.3*	*6.3*	*84.0*	*3.0*	81.4	7.6	88.9	9.2	*76.3*	*12.3*	*64.0*	*3.0*
27	70.3	5.8	77.3	8.8	82.7	8.3	89.7	3.7	80.3	6.4	83.1	11.3	*68.3*	*13.0*	*59.3*	*6.3*
28					*81.3*	*4.3*	*87.3*	*2.3*	75.3	7.3	79.4	9.4	*68.0*	*11.3*	*56.7*	*7.7*
29	74.5	5.0	75.0	8.0	75.7	10.3	87.7	7.0	*98.0*	*7.3*	*93.1*	*10.1*	*62.3*	*9.0*	*59.3*	*4.3*
30					*84.3*	*5.7*	*84.3*	*2.3*	*80.7*	*7.5*	*82.1*	*13.6*	*64.0*	*11.3*	*54.3*	*7.0*
31	74.7	14.3	78.7	7.0	85.0	14.0	86.3	4.3	*74.4*	*6.5*	*82.9*	*8.1*	*59.7*	*13.7*	*56.3*	*5.7*
32					*85.3*	*9.0*	*90.3*	*5.7*	*79.7*	*10.0*	*77.0*	*9.7*	*61.7*	*13.7*	*52.7*	*5.7*
33	69.8	4.3	73.5	8.0	71.7	3.7	74.3	3.7	80.0	5.1	81.0	6.6	78.7	9.7	63.0	8.7
34					*89.0*	*8.7*	*85.3*	*2.3*	73.1	5.2	75.0	7.4	68.3	7.7	57.3	2.3
35	66.3	4.8	72.7	5.3	79.0	13.3	88.7	0.4	71.0	6.4	74.4	7.4	68.3	8.7	58.3	5.0
36					*87.7*	*12.3*	*83.7*	*5.0*	67.3	6.4	73.7	10.3	66.7	10.3	53.0	4.7
37	64.0	3.0	66.7	7.3	82.0	12.0	90.0	4.0	*96.7*	*15.4*	*89.6*	*7.0*	64.7	11.0	53.7	6.3
38					*88.7*	*10.3*	*83.7*	*3.7*	*79.4*	*5.5*	*80.0*	*6.7*	63.0	9.3	55.3	3.0
39	66.3	4.3	72.0	6.5	77.3	9.7	89.7	3.0	*74.9*	*7.6*	*75.9*	*8.1*	62.0	2.7	52.0	6.7
40					*84.7*	*13.0*	*83.3*	*5.7*	*71.9*	*5.4*	*72.6*	*9.4*	62.7	8.3	53.7	3.3
41	69.7	2.3	70.2	7.3	75.7	18.3	90.3	4.7	91.9	6.0	89.4	10.0	*86.7*	*7.7*	*78.7*	*5.7*
42					*85.0*	*8.0*	*87.3*	*5.0*	77.7	9.0	83.4	7.7	*75.7*	*11.7*	*63.3*	*4.0*
43	67.3	5.3	68.5	4.0	81.0	9.7	88.4	6.0	73.9	9.0	80.0	7.9	*72.0*	*14.0*	*59.0*	*8.0*
44					*83.3*	*7.3*	*89.0*	*3.3*	66.7	7.0	75.3	12.0	*69.0*	*13.3*	*56.7*	*4.3*
45	63.7	4.3	71.0	7.5	77.7	9.0	86.3	3.0	*92.1*	*7.2*	*84.4*	*8.4*	*66.3*	*14.3*	*57.7*	*3.0*
46					*83.0*	*11.0*	*83.3*	*3.0*	*77.9*	*6.5*	*76.3*	*7.4*	*63.7*	*11.0*	*54.7*	*3.7*
47	64.7	1.8	69.5	7.0	77.7	9.0	82.7	4.3	*71.9*	*6.1*	*78.7*	*7.6*	*60.0*	*11.3*	*54.0*	*4.0*
48					*79.3*	*7.0*	*86.7*	*12.3*	*71.0*	*7.1*	*75.0*	*9.0*	*61.3*	*10.0*	*57.0*	*2.0*

TABLE VI (*continued*)

	Group I				*Group II*				*Group III*				*Group IV*			
	Men		Women		Men		Women		Men		Women		Men		Women	
49	64.3	1.8	70.7	11.3	71.0	5.3	68.3	3.0	75.1	5.0	74.6	5.3	70.3	5.0	64.0	9.0
50					*82.0*	*9.3*	*83.3*	*3.0*	66.3	6.4	70.3	7.6	61.7	6.3	58.0	4.7
51	67.7	4.8	69.7	5.8	73.7	8.3	78.0	2.7	63.0	5.5	68.7	8.0	61.3	8.3	55.3	2.3
52					*80.0*	*8.0*	*76.0*	*0.0*	61.4	6.6	68.3	11.4	61.7	7.0	53.0	4.7
53	62.0	2.5	68.5	5.0	74.3	9.0	79.0	4.7	*90.0*	*5.7*	*84.4*	*8.6*	60.7	4.7	53.0	5.0
54					*85.0*	*13.3*	78.7	*5.3*	*76.1*	*7.7*	*76.6*	*7.3*	60.3	10.3	53.0	3.7
55	67.3	3.3	66.7	6.8	81.7	14.0	81.7	2.3	*71.1*	*5.3*	*75.0*	*10.0*	58.0	6.3	52.7	4.3
56					*77.0*	*7.7*	*78.3*	*5.0*	*67.6*	*5.3*	*73.1*	*7.3*	56.3	9.7	55.7	3.7
57	62.0	3.0	66.0	8.0	72.0	11.0	77.7	1.7	88.9	10.7	89.3	11.7	*83.0*	*6.3*	*75.3*	*9.0*
58					*76.3*	*5.0*	*76.7*	*3.3*	77.1	5.3	78.8	9.1	*69.7*	*8.3*	*61.7*	*7.7*
59	62.7	1.8	68.7	7.3	74.7	12.3	79.3	0.4	72.1	7.3	75.0	11.1	*64.3*	*8.3*	*58.3*	*4.3*
60					*74.0*	*5.3*	*78.3*	*5.0*	68.3	6.4	75.3	12.0	*60.7*	*8.0*	*57.3*	*3.7*
61	65.3	5.8	67.0	9.0	73.0	12.0	80.0	4.0	*82.0*	*9.7*	*87.1*	*9.6*	*62.3*	*8.7*	*56.7*	*5.0*
62					*77.0*	*12.0*	*79.3*	*1.7*	*75.1*	*7.9*	*79.3*	*9.1*	*59.3*	*7.7*	*56.0*	*4.7*
63	64.3	3.3	65.0	7.0	73.3	11.7	80.7	5.0	*68.3*	*3.9*	*77.0*	*7.1*	*61.3*	*7.0*	*55.7*	*5.0*
64					*77.3*	*9.7*	*77.3*	*0.4*	*67.1*	*6.1*	*73.3*	*10.9*	*59.7*	*11.0*	*51.7*	*5.7*
65	69.3	4.8	70.2	8.8	70.0	6.0	67.0	3.3	72.7	4.6	75.4	8.6	72.3	9.0	61.7	6.3
66					*78.7*	*8.3*	*85.3*	*2.3*	66.1	6.1	69.9	7.3	61.7	7.0	53.3	6.3
67	66.5	4.5	63.3	6.8	72.3	11.0	82.6	6.7	62.3	6.6	67.9	9.6	63.3	10.3	53.0	5.7
68					*77.0*	*12.0*	*80.3*	*1.7*	59.1	5.9	68.1	10.0	59.0	9.3	52.7	3.7
69	63.5	6.5	63.0	4.5	71.3	14.7	79.6	2.3	*84.7*	*10.0*	*82.7*	*7.1*	57.7	9.7	52.0	4.7
70					*77.7*	*7.7*	*79.7*	*5.0*	*69.4*	*6.3*	*76.6*	*9.6*	60.0	7.3	53.7	2.7
71	61.3	4.8	64.0	4.5	70.7	11.7	80.0	0.0	*69.7*	*10.0*	*75.4*	*9.4*	60.7	11.0	52.0	3.3
72					*76.3*	*10.3*	*75.3*	*1.7*	*66.1*	*8.4*	*69.1*	*10.9*	58.0	12.0	51.3	3.0

TABLE VI (*continued*)

	Group I				Group II				Group III				Group IV			
	Men		Women		Men		Women		Men		Women		Men		Women	
73	62.5	4.0	61.0	3.5	73.7	12.3	80.7	4.7	85.0	6.3	82.7	6.4	*80.0*	*3.3*	*64.3*	*11.0*
74					*75.0*	*11.3*	*76.3*	*3.7*	74.7	4.9	73.0	9.1	*69.3*	*7.7*	*59.0*	*4.7*
75	59.8	4.3	64.3	4.3	71.3	10.3	77.7	5.0	71.0	5.4	71.3	8.0	*67.3*	*9.0*	*57.7*	*4.3*
76					*75.0*	*6.0*	*78.3*	*2.3*	67.6	7.4	71.4	8.0	*63.7*	*10.3*	*55.0*	*4.7*
77	62.0	4.5	62.0	7.0	72.7	13.7	77.0	3.3	*81.3*	*8.0*	*83.4*	*8.1*	*62.0*	*12.0*	*53.3*	*5.7*
78					*78.0*	*15.3*	*75.7*	*5.0*	*68.4*	*5.6*	*76.3*	*6.9*	*60.3*	*10.3*	*54.0*	*2.7*
79	60.7	5.8	61.5	5.5	75.3	12.3	77.0	4.0	*67.1*	*5.0*	*73.1*	*11.3*	*61.3*	*9.0*	*52.0*	*5.3*
80					*74.3*	*9.0*	*73.0*	*1.3*	*67.4*	*8.0*	*67.0*	*9.1*	*61.3*	*9.0*	*54.0*	*3.0*
81	60.7	7.3	57.7	4.8	74.7	4.3	68.0	3.7	67.0	7.1	71.7	6.3	66.0	8.0	62.3	8.7
82					*73.3*	*7.0*	*77.3*	*5.0*	61.1	7.0	66.9	8.4	61.7	12.3	54.0	9.3
83	58.3	4.3	58.7	6.8	72.0	10.7	71.3	2.3	62.9	8.1	65.1	7.3	59.7	10.3	52.7	5.0
84					*75.3*	*5.0*	*77.3*	*1.0*	60.3	7.0	65.3	10.0	60.7	12.3	51.3	6.3
85	62.7	6.8	57.0	4.5	69.0	8.7	75.0	4.0	*79.3*	*5.4*	*82.0*	*9.8*	54.0	6.0	48.7	4.3
86					*71.0*	*8.0*	*78.3*	*3.7*	*69.4*	*8.9*	*73.7*	*9.1*	55.0	7.3	49.7	5.3
87	60.5	7.0	60.3	8.3	69.3	9.7	74.3	3.0	*64.6*	*7.7*	*69.9*	*8.4*	55.0	8.7	49.3	3.7
88					*72.3*	*10.3*	*72.0*	*1.0*	*62.0*	*6.9*	*67.4*	*6.8*	55.0	7.3	48.7	5.0
89	57.3	8.3	56.7	6.8	71.0	10.0	72.3	3.0	82.6	8.6	83.7	9.4	*71.7*	*3.7*	*69.0*	*7.3*
90					*71.3*	*9.7*	*73.7*	*4.3*	70.9	7.3	70.7	11.1	*66.0*	*7.3*	*58.3*	*9.7*
91	59.5	4.5	58.0	9.0	67.3	8.3	73.3	1.7	67.0	5.1	67.7	9.3	*67.7*	*8.3*	*51.3*	*5.0*
92					*71.3*	*6.3*	*72.3*	*1.0*	63.6	6.6	67.9	10.4	*63.0*	*8.7*	*51.3*	*5.0*
93	60.3	4.8	54.0	4.0	69.7	8.3	76.0	6.0	*80.9*	*7.7*	*79.6*	*7.4*	*60.0*	*9.3*	*54.0*	*5.3*
94					*75.0*	*13.7*	*73.7*	*3.7*	*69.3*	*6.9*	*72.0*	*10.3*	*56.3*	*7.7*	*54.3*	*3.0*
95	60.3	7.8	53.7	3.8	69.3	9.7	74.7	5.0	*66.3*	*6.0*	*68.4*	*11.4*	*58.0*	*5.3*	*50.3*	*3.7*
96					*71.0*	*8.7*	*71.7*	*2.3*	*63.0*	*7.1*	*68.0*	*6.3*	*54.0*	*6.3*	*50.3*	*3.7*

table for purposes of comparison. There are no errors to deal with as all misthrows had to be corrected by the subject. All figures used represent time in seconds.

The curves as well as the tables show the interference effect and its relation to the practise effect, and indicate that the two associations are becoming automatic. All the interference curves of all the groups show that the interference is present but that it is overcome, and that the associations are becoming automatic. This is true for every individual in all the groups, although the individual curves of Groups II. and IV. are not shown.

In the alternate group the first sorting is about the same as that of the practise group. Among the women A_1 is 118 sec., A_2 is 123 sec., while in the practise series it is 120 sec. Among the men A_1 is 107 sec., and A_2 is 111 sec., while the practise group record is 117 sec. But the practise curve drops very rapidly, especially among the men, while the alternate curve does not show rapid improvement. The practise curve is concave while the alternate curve is a straight line descending from about 96 sec. to 72 sec. for the men. The women's curve is concave due to the very rapid improvement of the first day, after which it follows the men's curve closely. The rate of gain on the percentage basis is higher in the practise group than it is in the alternate group, being .36 for the men and .48 for the women in the practise group as compared to .29 and .37 respectively in the alternate group.

There are no distinct plateaus except that of My during the fourth and fifth day. On the sixth day he makes rapid improvement though he is uniformly poorer than the average of the group. But this plateau does not mean no improvement, nor that interference annuls practise, for on the first day he ranges between 100 and 115 seconds, while on the last day he remains between 80 and 90 seconds.

The first sorting of each day is decidedly quicker than any other and shows a decided drop in the curve. This decrease in time is because of the fact that the first practise has no interference, while for the rest of the sortings the interference of the opposing arrangement is present. The good initial records of each day indicate where the curve would be were it not for the interference. This shows that the two associations are well established and that the interference of the previous day has faded away. The women have a better initial record than the men, yet their general curve is slightly poorer. There seems to be a greater discrepancy between what they could do without interference and with it than there is among the men. These initial drops however both among men and women become relatively less and less and are hardly noticeable on the last day.

This is because of the fact that the records of the whole day are down to the standard set by the initial records, and that the interference has less and less influence. That they do not quite attain to the practise limit is obvious from the conditions of the experiment. When the change is made there is a temporary disadvantage to be overcome. This is often overcome in a few seconds, but it will always require a little time. There were individuals even in this group who sorted the eighty cards in 55 seconds, and for the entire sixth day did not go above 65 seconds. This is very near and in some cases below the practise limit. Em and Gr, although the poorest of the groups, kept under 70 seconds for the entire sixth day.

In Plates II. and III. and in Table V. we have the individual curves and records for Group III. The average curve is given in Plate IV. The average curves show three spires each day showing clearly the changes in arrangement. At the beginning of each day there is no rise of any account in the curve, which begins on the practise level of the previous day, no interference effect being retained. The first arrangement without interference attains as low a point in the curve as is attained on that day and with the women a little lower. Towards the last the best absolute time is made in the first four without interference, but this is very small and with some subjects nothing at all. It may be because of being perfectly fresh at the experiment. The curves show that the improvement is very rapid after the changes and that the same efficiency is reached as before.

Although the women's practise curve is a little better than that of the men, their curve in Group III. is not quite so good, which is most plainly shown in the last few days by the drop in the men's curve. The men's curve surpasses their practise curve quite frequently but the women's does not, except at first.

For the first three days the men rise higher after the change than the women, for the last three days they are about the same. In spite of this higher rise of the first three days and the equivalent rise of the last three, the men attain a greater efficiency than the women, shown by the fact that their curve comes lower than the women for the last sortings of each section. The four women in the lower section of Plate II. do not show the changes in arrangement so clearly. There were many cases where the last sorting of a section took longer than the first. Among the men there were very few cases where the second sorting of an arrangement took as long as the first; among the women there were many where it took longer. During many of the sections there is negative improvement among the women; never among the men. Of course the women do not rise so high for the first sorting after the change but on the other hand they do not im-

prove so rapidly. The previous association seems to be made automatic by the men more quickly than by the women.

The average curves show that by the second sorting the same plane of efficiency is reached as had been attained before the change. This allows the two remaining sortings to accumulate practise effect and reach a better level. The men's curve passes that of the women, being a little higher on the first day, but on and after the second day it is lower than that of the women. Both among the men and the women the rise after the change becomes less and less, and is not nearly so great on the last day.

The interference becomes less and less, the two associations become automatic, and need only the first sorting to reduce them to the practise level and overcome the temporary disadvantage. The improvement on the percentage basis for the first arrangement is .29 for the men and .27 for the women; for the second arrangement .34 for the men and .25 for the women. The curves are slightly concave, nearly as much so as the practise curve, and in this are differentiated from those of the alternative group which are straight lines. The best record in Group III. was that of Ca, 44 seconds, which was one second less than the best in the practise group. Thus the best record is in the group having interference.

Mrs. Sv and Mr. Sp are the only subjects showing plateaus in Group III. Sp makes no progress after the third day and is decidedly poorer on the sixth day. His performance on the first day averages about 110 sec. with a wide range, while on the last day he varies between 85 and 95 seconds. It is noticeable that at his best he rises high immediately after the change, while on the sixth day he does not rise so high, but neither does he reach such a low level.

Mrs. Sv varies from 110 to 125 seconds at the beginning, but on the last day she ranges between 85 and 95 seconds. Though she makes the least improvement she by no means stands still. Often after the break she does nearly as well as and sometimes better than before, but her improvement is low within the sections and many times negative.

The average records of men and women in Group IV. are shown in Plate V. The change to the second arrangement is very clearly shown every day while there is just a little rise at the beginning of the day. Here the record of the women is a little better than that of the men, due to the poor records of As among the men and to there being only three subjects. Moreover the work of Misses Mor and Ag was exceptional among the women. Miss Ag made the best record of the entire experiment, sorting the eighty cards in 42 seconds, and having many records below 50 seconds. After two or three days the time was as good for the second arrangement with interference as

for the first without it. As and Mrs. Th were much poorer on the second arrangement than on the first for the first three days, after which the two were about the same. In this group the men again seem to rise a little higher after the change but it is not nearly so pronounced as in Group III.

Group IV. shows a higher rate of improvement than Group III. (.35 for both men and women in the first arrangement and .34 for both in the second arrangement). The curve is concave and is as good as the practise curve except immediately after the changes. Here we find a much more rapid automatization of both associations, and interference playing less and less a part. The interference effect of eight previous sortings seems no more than that of four. What relation the number of repetitions have to the interference effect will come up for discussion under the head of "Relation of Interference to Practise Effect."

Bair asked the question whether the interference becomes greater with the number of practises of the previous association,[2] that is, whether a habit made more permanent with many practises offers more resistance than one formed by only one or a few practises. He found that the difference between the last sorting of the first series and the first sorting of the second is slightly greater with many repetitions of the first. He concluded, however, that this was due not to interference but to the decrease in the first order. His results showed that it took less time to make the associations in the second order after practising the first order many times than with only a few practises of the first order.

Table VII. summarizes the results under this heading for all the groups. This gives us a change to the new order after one order had been fixed by one, four, eight, and forty-eight repetitions. The data of Group I. were obtained by having the subjects sort the cards into the second arrangement after their forty-eight sortings of the first arrangement. The data of the other three groups are the records of each group for the first sortings of each arrangement on the first day of the experiment.

We find that in Groups II., III., and IV., where one, four and eight sortings of the first order were given before changing to the second, the first performance of the second order takes longer than the first performance of the first order. But we find in Groups III. and IV. a more rapid improvement in the second arrangement than in the first. This is due to the transfer effect of the sortings of the first order. The higher rise then is due entirely to interference but the interference is very rapidly overcome. Bair found that with ten

[2] J. H. Bair, *loc. cit.*, p. 37.

practises of the first order the second took 1 second less. Group IV. takes just four seconds more. Bair had just half as many cards, so that the difference between his results and the present are very

TABLE VII

The upper column gives the first four sortings of the one arrangement, except in the alternating group, where only one could be obtained; the lower column gives the first four sortings of the new arrangement, except in Group II., where only one sorting could be obtained.

Group I		*Group II*		*Group III*		*Group IV*	
Men	Women	Men	Women	Men	Women	Men	Women
117	120	107	118	118	114	101	93
95	105			100	101	91	78
84	101			94	97	74	75
81	104			89	93	73	69
			Change to New Order				
120	107	111	123	132	118	104	97
102	98			108	99	87	81
93	90			98	90	82	72
86	85			92	86	76	67

small. In the practise group which had forty-eight sortings of the first order before changing to the second, the results tally with Bair's. The men's time increases just 3 seconds while that of the women decreases 13 seconds, which on the average (as Bair takes it) brings the time a little less for the changed order than for the first. In Group I. the women improve much more rapidly after the change than in the first four of the first order, but the men have about the same rate of improvement.

TABLE VIII

Group I		*Group II*		*Group III*		*Group IV*	
Men	Women	Men	Women	Men	Women	Men	Women
60	53	4	5	43	25	39	34

Table VIII. shows, as Bair found, that the difference in time between one sorting and another requiring antagonistic reactions is greater, the greater the number of sortings of the first arrangement. We find that the greatest difference is in the first group, the next greatest in the fourth group, the next in the third group, and only a small difference in the first group. The difference is greater with the increasing practises of the first order. But as shown above this increasing difference is due to the better records attained in the first order and not due to interference.

These results fully agree with those of Bair, with the additional fact that the second arrangement shows greater rapidity of improvement.

CHAPTER IV

INDIVIDUAL DIFFERENCES

1. *Rate of Improvement*

An important question in both experiments is that of the amount and rate of gain. What is the rate of gain in the card sorting experiment when interfering associations are present as compared with that of simple practise? What is the rate of gain in the typewriting experiment after the break as compared with that before? Are the differences among the group greater or less when interfering associations are present?

Table I. gives the initial performance, final performance, absolute gain, and rate of gain in the typewriting experiment. Table X. gives the initial performance, final performance, absolute gain, and percentage of gain, for each of the arrangements in all the groups of the card sorting experiment. The accompanying table gives the averages, medians and variability of the several groups in both experiments. The variability is expressed by the Pearson coefficient of variability. The group marked "Sectional gains of Group III." is obtained by finding the average improvement of the sections of four sortings of each arrangement. The difference between the first and fourth sorting is divided by the first sorting. By taking the average of the twelve sections for each arrangement (there being two sections each day) we have a fair measure for purposes of comparison as to how much gain is made in the individual sections. This has no significance for the total gain, but it has value as a measure of the immediate rate of improvement when four repetitions of the previous association are present. The median is used for the purpose of comparing the groups and the average for the determination of the variability. Only in a few cases are the averages and medians different.

We find that 28 per cent. of the men of the typewriting experiment reach or surpass the median of the women both before and after the break. In the practise group of the card sorting experiment 25 per cent. of the men reach or surpass the median of the women. In the alternating series no men reach the median of the women. In Group IV. one third of the men reach the median of the women. These groups are however too small to warrant any conclusions as to sex differences.

TABLE X

	Initial Performance		Final Performance		Absolute Gain		Percentage of Gain	
	A_1	A_2	A_1	A_2	A_1	A_2	A_1	A_2
Group I								
Wa	103		53		50		.48	
Co	82		54		28		.34	
Ol	100		70		30		.30	
Br	91		60		31		.34	
Miss Sm	115		56		59		.52	
Miss Co	85		47		38		.45	
Miss Fo	127		67		60		.47	
Miss St	104		53		51		.49	
Group II								
Em	85	95	60	65	25	30	.29	.31
Gr	97	102	65	65	32	37	.33	.35
My	108	113	83	86	25	27	.23	.24
Mrs. Cu	100	108	67	74	33	34	.33	.32
Miss Br	123	111	76	75	47	36	.39	.32
Mrs. Mt	120	130	70	69	50	61	.41	.47
Group III								
Ca	83	92	57	65	26	27	.31	.29
Si	100	120	73	73	27	47	.27	.38
Dr	97	103	69	68	28	35	.29	.34
Wi	93	102	67	65	26	37	.28	.36
Sv	93	84	66	63	27	21	.29	.25
Sp	119	126	90	89	29	37	.24	.29
Rd	117	126	72	67	45	59	.38	.47
Miss Br	118	110	74	70	44	40	.37	.36
Mrs. Kv	103	111	77	81	26	30	.25	.27
Mrs. Wo	86	83	61	60	25	23	.29	.28
Mrs. Sv	122	116	97	91	25	25	.20	.22
Mrs. Su	88	85	66	66	22	19	.25	.22
Miss Th	102	98	73	70	29	18	.28	.18
Mrs. Ao	90	86	65	65	25	21	.28	.24
Group IV								
As	91	100	64	68	27	32	.30	.32
Cu	75	76	44	47	31	29	.41	.38
Bl	86	86	56	56	30	30	.35	.35
Miss Mor	76	76	51	51	25	25	.33	.33
Miss Ag	68	75	42	47	26	28	.38	.37
Mrs. Th	95	86	45	58	30	28	.32	.33

	Men				*Women*			
	Av.	A.D.	Med.	Var.	Av.	A.D.	Med.	Var.
Typewriting, before break	.72	*.04*	.73	*.47*	.745	*.05*	.76	*.58*
Typewriting, after break	.50	*.07*	.50	*.99*	.57	*.07*	.58	*.93*
Prelim. Group before break	.67	*.03*	.67	*.37*				
Prelim. Group after break	.53	*.12*	.53	*1.59*				
Card-sorting Group III.—A_1	.29	*.03*	.29	*.55*	.27	*.03*	.28	*.57*
Card-sorting Group III.—A_2	.34	*.07*	.34	*1.20*	.35	*.04*	.24	*.80*

Sectional gains of Group III.								
A_1 without interference ...	.16	*.05*	.14	*1.25*	.12	*.04*	.10	*1.14*
A_1 with interference	.25	*.04*	.26	*.80*	.16	*.07*	.13	*1.75*
A_2	.23	*.05*	.25	*1.04*	.17	*.06*	.15	*1.46*
Practise Group	.36	*.05*	.34	*.83*	.48	*.02*	.48	*.29*
Alternate Group	.29	*.04*	.29	*.75*	.36	*.05*	.35	*.83*
Group IV.—A_1	.35	*.04*	.35	*.67*	.34	*.02*	.34	*.34*
Group IV.—A_2	.35	*.02*	.35	*.33*	.34	*.02*	.34	*.34*

In Group III., however, there are seven men and seven women, and the interference effect is most clearly shown. Both in the absolute gain and in the percentage of gain Miss Br alone surpasses the median of the men in both arrangements. In A_1 Miss Th surpasses the median of the men. When it comes to sectional gains, Mrs. Su alone reaches or surpasses the median of the men in both arrangements, and Mrs. Ar surpasses their median in A_1 only.

We find the women just a little ahead in the typewriting experiment but not sufficiently to cover the probable error. The variability of the two groups is about the same. Both the men and the women in the regular and preliminary groups have greater variability after the break than before. In the regular group of the typewriting experiment the variability after the break is 1.60 greater than before, and for the men 2.11 times as great as before. In the men of the preliminary group the variability is 4.3 times as great after the break as before. This index of variability shows greater differences in improvement when an old association is persisting than without interfering associations. The trend of the entire series of experiments emphasizes this greater variability under conditions of interference.

Another significant fact of the rate of improvement in the typewriting experiment is the lack of correlation between the rate of improvement after the break and before. Were there such a correlation it would indicate that the differences found after the break are simply due to the ability of the subject to improve by practise as the simple practise curve shows. Among the men of the preliminary group there is a negative correlation of —.86 (P.E., .05); among the men of the regular group a correlation of .13 (P.E., .19); and among the women a positive correlation of .45 (P.E., .17). This indicates that the ease and rapidity of improvement after the break is not exclusively due to the same ability as the improvement before. The astonishing fact is that some of the writers before the break, who expected to make great progress after, did not nearly measure up to their expectations, while others did a great deal better after the break than before. Cl was the most rapid writer before the

break, but did very poorly after it. On the other hand Ru who had very poor records before the break became considerably more efficient after it and did not seem to be much disturbed because of the change. Of course a negative correlation of —.86 is exceptional, but the tendency seems to be practically no correlation.

Under the study of relapses is brought out the fact of correlation between efficiency in improvement and avoiding mistakes, which is also important as an index of individual differences.

In the card-sorting experiment we find but little difference as to variability in the rate of improvement. The men are more variable in the simple practise, and less variable in the alternate and sectional measures of Group III. These are the only well-defined sex differences. In both these groups the improvement is greater among the women than among the men. As would be expected the improvement in the practise group is greater than in the alternating group, the difference being .07 for the men and .12 for the women.

In Group III. the rate of improvement is higher than it is in the alternate group. This is especially true of the second arrangement which is continually subject to interference. The differences are greater in the sectional measures of the four sortings of each arrangement. These measures are the average rate of improvement for the sections of four sortings. Here the median percentages under interference are for the men .26 in A_1 and .25 in A_2; and for the women .13 in A_1 and .15 in A_2. Since this sex difference is greater than the sex difference in the total rate of improvement, the men must rise higher in the curve after the change and thus be able to improve more rapidly. This is the actual fact and explains the more rapid gain. The curves of Plate IV. show that the men drop lower with practise and rise higher immediately after the change. But this fact is still more clearly shown by the actual difference in time between the last sorting of the one arrangement and the first sorting of the next. This difference is 15 to 20 seconds greater for the men than for the women. This explains the fact that in the sections the men's rate of gain on the percentage basis is about twice as large. After the third day the men do not rise much higher immediately after the change, but they improve more than the women.

This greater immediate interference among the men does not seem to the writer to indicate that four sortings have a greater interference effect on the men than on the women. The men and women who had the least rise after the change had the poorest records, while those who rose high also dropped very low. With the poorer ones neither association seems so well fixed. Mrs. Su and Ar, who made the best records of the women of Group III., rose very high

immediately after the change, but improved rapidly. Miss Ag, who had the best time of any subject, rose very high after the change, but showed rapid improvement. Mrs. Sv and Ku who had the poorest records among the women of Group III., did not rise high, in fact, often did better right after the change than on the fourth sorting, thus giving a negative rate of improvement. It does not seem true that interference has the most lasting effect on those on whom it shows up strongest immediately.

The indication of the experiment is rather that those who rise highest after the change can most easily overcome the interference. It seems to the writer that interference effect must be measured by its *persistency,* and that those in whom it persists the longest are the most subject to interference. If this is true, the women of Group III. are less able to overcome the interference than the men. The men show a greater immediate effect but it is rapidly overcome. The reason the men can overcome the retarding effects of the previous associations more fully is because the two associations are more firmly fixed and more nearly automatic, so that a few sortings will raise the association to its previous efficiency. But why should the men show this greater immediate and momentary interference? Because of the fact that both associations are more firmly fixed and will when in full play require more energy to be displaced and give the momentary advantage to the other association. These higher rises indicate better fixed and more automatic associations, and the smaller effects of the change indicate less well fixed associations. When an individual on the fifth or sixth day of the experiment fails to improve with four sortings, the indications are that both associations are weak. When an individual however shows a great immediate effect but a rapid gain, it indicates that both associations are well fixed. The interference has the greatest effect where no associations are well fixed, for it is the cause that neither is well fixed. With the other individual, interference is an *incident,* but not fundamental. The fundamental thing is the fixing of the two associations.

Through the entire group the individuals that improved most and reached the best absolute time were those that rose high after the change but improved rapidly; those who improved least for the whole experiment and who did not reach the level attained among the others, were those who did not rise high nor improve rapidly. If the above explanation be the correct one, the men's associations are better fixed and interference has less influence over them. Their improvement in the sections is twice as great, the first performance after the change is 15 to 20 sec. poorer than that of the

women, and they reach absolutely lower levels. These differences are also true within the groups.

I do not however think that we are justified in drawing any sweeping conclusions as to sex differences on this point. Aikins[1] says that men have a greater tendency than women to form habits and use them, to build them together and to build one on another, to reconstruct the personality on the basis of habit. This he calls a truly secondary sexual characteristic and it is one of the arguments why woman is more primitive than man. Building up a personality on the basis of highly elaborated and varied habits is certainly a criterion of development. Well-fixed associations would indicate a higher stage of organization, and a greater freedom from overpowering interference than weak associations on which the interference would have great effect. The men in the experiment seem to have better fixed associations, a set of habits better developed, than the women have. The experimental results agree with the contention of Aikins. If these conclusions are correct we should expect men to have stronger habits and associations, and better able to build up the personality on the basis of habit. With them interference would have less effect. Interference would have a greater effect on the women. The men should therefore be better able to adapt themselves readily to the variations which the environment presents.

It must be borne in mind, however, that the sex differences here are not large, not nearly so significant as the individual differences among both men and women.

2. *Initial Efficiency and Plasticity*

One of the interesting questions of individual differences in mental traits is that of the relation between initial efficiency and susceptibility to improvement by practise. Does a high initial efficiency indicate a greater ability to profit by practise or does it indicate that the individual is farther along on his practise curve, and therefore, not so susceptible to improvement by further practise? Conversely, does low initial efficiency indicate less ability to improve by practise, or is the slow individual simply not so far along on the curve and therefore susceptible to greater improvement? F. L. Wells[1a] asks the question thus: "Is high initial efficiency a product of greater amount of practise, or of greater ability to profit from practise, and conversely, a low initial efficiency the product of lack of practise or of little ability to profit from practise? As the

[1] H. A. Aikins, "Man, Woman and Habit," *Psy. Bull.*, **5**, 50, 1908.

[1a] F. L. Wells, "Relation of Practise to Individual Differences," *Am. Jour. Psych.*, **23**, 75, 1912.

first or second of these alternatives hold true we may expect less or more improvement by the more efficient.''

Wells conducted practise experiments on men and women in a simple addition test and in a number checking test. With few exceptions the curves do not cross. Those having the highest initial efficiency have the highest final efficiency, and those with the lowest initial efficiency have the lowest final efficiency. There is a fair correlation between the relative positions in initial and final efficiency. He concluded thus: ''We are confronted then with cases indicating a high initial efficiency as a manifestation of superior ability to profit by practise, or plasticity; and on the other hand with cases exhibiting a low initial efficiency with minor possibilities of practise improvement.''[2]

A high correlation exists between the initial efficiency and final efficiency in the card sorting and typewriting experiments, and in the card-sorting experiments there is a correlation between the initial efficiency and the rate of improvement. This shows that not only are those with highest initial efficiency able to improve as much, but even to improve more. With this it must also be borne in mind that those with poor initial records tend naturally to get a high percentage of gain.

In Group IV. of the card-sorting experiment there is a perfect correlation in relative initial and final position in A_1 and A_2 among both men and women. There is also perfect correlation between initial position and rate of improvement in both A_1 and A_2 among both men and women. These figures are all given in Table X., which show initial and final efficiency and rates of improvement for all the subjects.

In Group II. there is a perfect correlation between the relative initial and final positions of the first arrangement among both the men and the women. In the second arrangement the Pearson coefficient for the men is 1.00, and for the women .50 P.E. .29. The men have a correlation of .50 P.E. .29 between the initial efficiency and the rate of improvement in both arrangements. There is no correlation between the initial efficiency and rate of improvement among the women.

In Group III., with seven men and seven women, the correlation between the relative initial and final position of A_1 is .96 P.E. .02 for the men, and .93 P.E. .03 for the women. In A_2 it is .89 P.E. .05 for the men, and .96 P.E. .02 for the women. Between the initial efficiency and rate of improvement of A_1 the correlation for the men is .43 P.E. .20. Only one man and 2 women out of the fourteen

[2] Wells, *loc. cit.*, p. 81.

subjects are out of their relative position, though there is not much correlation between initial position and rate of improvement. The outstanding exception among the men of Group III. is Rd, who has the poorest initial record but whose rate of improvement is highest. His low initial record seemed to be due to a peculiar nervousness and hesitancy. He soon seemed at ease in the experiment. He was the only subject showing a very poor initial record and high improvement.

Among the women of the practise group there is perfect correlation between the initial and final efficiency, but none between the initial efficiency and rate of improvement. Among the men of the practise group there is no correlation between relative initial and final position. Wa is the poorest at the beginning and the best at the close.

If we take the results of the two arrangements of Groups II., III., and IV., and the one arrangement of Group I., we find a total of 60 cases of initial performance with a practise of 48 repetitions. Out of these sixty only eight curves cross any others. Six of these eight cross only one curve, one crosses two, and one crosses three. Of the six who end just a little better than the one starting next below them three of them are only one second lower. This makes it almost universal with the cases in hand that a higher initial efficiency goes with a higher final efficiency and a lower initial efficiency with lower final efficiency.

We find the most significant exception to this in the practise groups. Among the men of the practise group there is no correlation. In none of the practise groups is there correlation between initial efficiency and rate of improvement. In all the groups having interference we find high correlation between relative initial and final position, and some correlation between initial efficiency and rate of improvement. In Group IV. it is perfect both among the men and women. In Group III. the correlation is high for relative position and .68 between initial position and rate of improvement of the first arrangement for both men and women. In Group I. there is high correlation in relative position and .50 between initial position and rate of improvement for the men in both arrangements, 0 for the women. We find a higher correlation between initial and final efficiency in the interference groups than in the practise group. We also find no correlation in the practise group between initial position and rate of improvement, while we do get several appreciable correlations in the interference groups. It seems then in a test such as the card-sorting experiment, which demands a high degree of attention and quick discrimination, the most efficient are suscept-

ible to not only as great but even to greater improvement than the less efficient at the beginning. And when interference is continually present there is still a higher correlation between plasticity and initial efficiency. Not only are the more efficient able to improve more in pure practice but when the opposing factor of interference comes in their ability comes still more clearly to light. This correlation of the plasticity of the individual, his ability to adapt himself to the new situations and overcome the ever-present interference, with his initial ability is significant, and gives a clearer and more pronounced index of individual efficiency than the simple practise curve can ever give.

Table I. gives the initial and final performance of the typewriting experiment both before and after the break. We find some correlation between the initial and final efficiency, both before and after the break. In the preliminary group the eight men have a correlation of .95 P.E. .02 before the break, and .76 P.E. .09 after it. Among the men of the regular group it is .96 P.E. .02 before the break and .43 P.E. .20 after the break. Among the women the correlation is .60 P.E. .14 before the break and .50 P.E. .17 after the break. There is no correlation worth mentioning between the initial position and rate of improvement. Here we find a distinctly higher correlation before the break than after it. The correlation among the men is higher than that of the women. The smaller correlation after the break is due to the greater variability of improvement which we found after the break. This shows that the break has a very great initial effect on some, but is soon overcome, while others do not show such a great immediate effect but a more persistent one. This same thing is shown by the relapses, rate of improvement and variability, and is clearly seen in the curves.

3. *Errors as a Cause of Interference*

Most investigators of the practise curve have pointed out the grouping and individual differences in errors. Yoakum says: "Perhaps the most noticeable individual difference is in the errors and their grouping. They were nearly always found in well limited groups with periods free from errors.[3] Swift and Schuyler also pointed out that the errors come in bunches and that there are long periods without any errors.[4] Book says that there is a direct correlation between the errors and the fluctuations of attention.[5] Again

[3] C. L. Yoakum, "Experimental Study of Fatigue," *Psy. Rev. Mon. Supp.*, **46**, 77, 1909.

[4] Swift and Schuyler, "The Learning Process," *Psy. Bull.*, **1**, 310, 1907.

[5] Book, "Psychology of Skill," University of Montana Publications, 1, 118.

he says, "blunders, false associations, and bad habits are liable to repetition."[6]

Outside of these remarks there is no analysis of these groupings of errors. What is the nature of the groupings? Are there various kinds of errors or repetitions of the same error? Are there individual differences and if so what is their relation to the general performance? What is the cause of the grouping, and why is this cause not operating during periods that are free from errors? Book continually hints at something which may give a clue but which he does not develop. He uses such expressions as "interfering habits, associations, and tendencies," "bad associations," etc., very frequently throughout his discussion of errors. These interfering associations and tendencies may have a very important bearing upon the grouping of errors with reference to their nature and effects.

TABLE XI

For explanation of table, see text.

	1	2	3	4	5	6	7	8	9	10	11	12	13	15	20
Women:															
Ha	51	25	10	2											
			4	3				1			1		1	1	
Mo	27	8	1	1	1										
			1	1		2									
Ta	22	9	2												
			2	1			1								
Hf	11	3													
			1												
McC	34	28	29	4	1	1			1						
			3	2		1		2	2	1	2				
Wa	36	23	4	1	1			1	1		1				
			4	2		1		1							
Wh	10		1												
Og	23	8	1	1											
			3	1											
Gr	31	3	4	2		1									
			1		1	1									
Men:															
Cl	22	11	1												
			3												
Ru	22	13	5		2										
			5	1											
Ha	43	21	10	5	1	2									
			7	2				1					1		
Br	27	1													
			1												
De	25	7	1		1										
			1	1											
La	31	7	1												
			2	1											
McC	20	22	5												
				1			1								1

[6] Book, *loc. cit.*, p. 82.

TABLE XII

For explanation of table, see text.

	1	2	3	4	5	6	7	8	9	10	11	14	15	26	30
Women:															
Ha	1				1										
Mo	4	1	1			1									
Ta	4	1			1										
Hf	6		2												
			1												
Wa	25	11		3											
						1									1
McC	19	5	4	5	1										
			1	1						1		1			
Wh	2														
Og	11	1	2	5			1	1							
									1						
Gr	15	3	4	3	1	1									
			1			1							1		
Men:															
Cl	5	1	3												
					1										
Ru	8	2		1											
					1										
Ha	13	6		1	2										
			2	1											
Br	15	2	3		1										
				2	1										
De	11	2													
			3												
La	12	3													
McC	25	7	1												
														1	

Table XI. gives the actual grouping of errors among the seven men and nine women of the regular typewriting experiment. I omit the eight men of the preliminary experiment because of the slightly different conditions. The errors given are those committed in the learning process during the 130 repetitions before the break. They do not include errors of omission for the key may have been struck but not hard enough to register. Nor do they include repetitions of the correct letter for that may often be due to a superabundance of movement especially in the early stages. Only where a wrong letter is struck is the error counted.

The figures in the upper horizontal column record one thing and those of the lower horizontal column another thing. The upper horizontal column gives the number of trials or repetitions which

contain a given number of errors. The figures at the top of the table give the number of errors in a trial, and the figures in the horizontal column give the frequency of the trials containing that number of errors. To make it perfectly clear the table shows that the first woman, Ha, has 51 trials each containing one error, 25 trials each containing two errors, 10 trials each containing three errors, etc. It will be remembered that there are 21 letters to be struck in each trial, and that there were 130 trials. If the horizontal column be added we have the total number of trials containing errors. If the figures of the column be multiplied by the number of errors they contain and their products added we have the total number of errors. The tables are not intended to show the totals so much as the distribution.

Turning now to the second horizontal column of the table we have another fact expressed. In reading this part of the table the figures at the top of the table give the number of successive trials each containing one or more errors. The figures of the horizontal column give the frequency of such groups of successive trials containing errors. Turning again to Miss Ha we find that she has four groups in which three successive trials contain errors, three groups where four successive trials contain errors, and one group each of eight, eleven, thirteen, and fifteen successive trials, all containing errors. This shows the tendency of the subjects to have a run of errors in successive trials. In this column no figures are given under 1 and 2 because one or two trials does not give a succession of trials. Table XII. gives the grouping of the relapses after the break in the same way.

Turning now to the upper horizontal column we find great differences in the number of errors per trial. Miss Wa has trials containing eight, nine, and eleven errors each. Br and Miss Wh have only one trial with more than one error. All the subjects except Mrs. McC have a majority of their trials with only 1 error each, Mrs. McC has 34 trials with 1 error and 64 with more than one error. The total number of errors varies from 13 for Miss Wh to 207 for Mrs. McC; the men vary from 29 for Br to 152 for Ha. The number of trials in which the errors occur varies from 11 for Miss Wh to 98 for Mrs. McC; while the men vary from 29 for Br to 82 for Ha. We find that some have an average of over two errors per trial while with others it is just a little over one. The variation is about 2 to 1.

Turning now to the lower horizontal column which gives the frequency with which a succession of trials with errors occur, we find more marked individual differences. Miss Wh has no groups. Br,

De and Mrs. Hf show each one group of three successive trials containing errors, and De shows one of four. On the other hand Miss Ha has separate groups of thirteen and fifteen lines containing errors, while Mrs. McC has a number of eight, nine, ten, and eleven successive trials. McC has one group of twenty successive lines with errors while Ha has one of thirteen. This shows the tendency of errors to repeat. When some individuals make a mistake in one repetition the succeeding repetitions are liable to have errors, with others this is not so true. The table shows the extent to which it is true among the subjects.

Table XII. gives the same information for the relapses. The same facts here are evident though in not so pronounced a form. Here there are of course only fifty trials, and the totals are not nearly so large. Miss Wa and Miss Og show about the same number of relapses, but Miss Og in about half as many trials. Miss Ha with only six relapses has five of them in one trial. Some average three relapses per trial others only one. Turning to the second horizontal column we find that Miss Wa has an extraordinary repetition of relapses, having thirty consecutive trials with relapses. McC has 33 trials containing relapses, but 23 of these come in succession. Mrs. McC has 31 trials containing relapses and 31 of these come in groups. Miss Gr has 24 of her 27 trials containing relapses in groups. Others have no groups. This shows the tendency of one relapse to bring others in the succeeding trials.

It seems to the writer that there is a fundamental difference between the two kinds of grouping shown in the tables. The one kind of grouping is that of several errors in the same trial, the other that of a number of successive trials containing for the most part only one or two errors each, but repeated for many trials. The records as well as the introspections show that one error tends to cause another. This is what Book means when he speaks of "bad associations," "bad tendencies" and "interfering habits." Errors are not grouped merely by chance, but an error makes an interfering association which is hard to overcome. The cause of the grouping of errors is due to these bad and interfering associations. Book finds that in some cases it takes a very long period of time for them to drop out, and that not until they do drop out can there be unimpeded progress. He cites the case of a music teacher who recommends that in learning to play a piano, it is better to practise in periods of 15 minutes with recesses, than to continue for an extended practise, because of the fact that interfering tendencies do come in and cause error. When these drop out gain can be made without the strain and useless labor that would be expended in a

long practise at the time. This accounts also for the little improvement on some days and the smooth progress made on other days.

Where many errors occur in the same trial we have what I shall term "general" interference due to the bad association. A wrong reaction may bring on four or five incorrect reactions, none of them a repetition of the first but all different and due to a sort of confusion brought on by the first error. Many observations show that a single error destroyed the freedom of movement, broke up the general association connection, misplaced the confidence of the subject, and had a general inhibiting effect, causing various errors in the same trial. Often the subject would completely recover and on the following trials make no errors or only incidental ones.

But where a wrong reaction occurs in a given trial and is repeated in the succeeding trials, giving us a large group of lines containing the same errors, we have "specific" interference of the bad association. Miss Ha made one wrong reaction 18 and another 22 successive times on the same day, while generally she made comparatively few errors, and at no time did she make many errors in one line. She was told that she was making an error, but never suspected what it was and was surprised at the result at the end of the experiment. This is a very pointed case of "specific" interference, and yet never does the repeated error show any bad effect in the general sense upon the other reactions of the same trial. The same subject made only six relapses. Immediately after the break she arose to the situation, but other errors crept in and persisted without correction to the end. The table shows that she had many repetitions of the same error.

On the other hand, Miss Wa has many trials containing 2, 3, 5, 8, 9, and even 11 errors, but no repetitions in successive trials until after the "break" one relapse repeats itself a number of times. One error may upset her for the whole trial, causing great confusion, lack of confidence, slowing up of speed, deranging the association and generally inhibiting the work. There may be errors in the next line but not necessarily a repetition. She has a very marked general interference of the error but no specific interference.

Hf of the preliminary group (no table is here given for them) made more errors than any other subject, but the effect of an error was not specific but general, nearly always bringing others. He had hardly any trials with only one error. Ja of the same group did not show general interference but specific, once having a run of 17 repetitions of the same error.

McC presents a marked case of specific interference with a gen-

eral inhibitory effect which lowered his speed. After the "break" he had a run of 26 relapses and before the "break" a run of 13 repetitions of an error. This error and relapse did not cause other errors for he rarely made more than 2 errors or 1 relapse in a trial. The former habit had apparent control of this particular reaction, for after each relapse he would say he had made a mistake but did not know what and did not think it was the same one as before, being quite surprised later to find out that it was. The error seemed to make him very deliberate. The observer could in practically every trial see him about to strike the relapse number, then shake his head and strike the correct one. Sometimes he was about to strike the wrong one several times. Many times he would hesitate, even moving his fingers several times before striking, although he had the right reaction in mind. He said that his inhibitions were very strong and that he could not attain great speed on account of the mistakes. We see here that the specific interference had great control over him while the general effect was to increase his time but not to cause errors.

These bad associations take many forms. In writing the three-place numerals inversions often occur. There are repetitions. 639 was repeated instead of 628, the "6" giving the cue to the previous association. Sometimes the "3" of 853 was changed to "1" following the association of the "1" with the "5" in the previous 751. Sometimes also a number farther on was placed back and then correctly repeated in its proper position. Most of the errors however are the association of an absolutely wrong number with the reaction and then its repetition.

The individual differences in grouping may seem to be minimized by the fact that those making the greatest number of errors are those having more errors per trial and more groups of successive trials containing errors. This is of course what will happen from the law of averages. But why do some make more errors than others? And why do the errors tend to form such groups? We can not separate all the factors which go to causing errors in the learning process. But one of them seems clearly to be this general and specific interference. An error is likely to happen for some cause or other, but whether that error will cause others or not may be a matter of individual difference. The evidence at hand seems to point to that conclusion. With some individuals an error does not seem to have bad interfering effect, with others it has a general interference effect, with still others a specific effect. That they both operate to some extent is evident, but that one operates more strongly in one individual and the other in another, is also evident.

The fact that there is no correlation between the errors and relapses shows individual differences in meeting one kind of interfering associations which are not the same as those meeting another kind of interfering associations.

The overcoming of such interference is important in all forms of the learning process. Much of the delay and lack of progress, and to a large extent the plateaus themselves, are due to such a combination of bad associations and tendencies, which must be overcome. Whether their effect be to cause general confusion and lack of control or repetition of the specific error, they must be reckoned with. Suffice it to say here that a closer analysis of errors is needed for the purpose of offsetting their bad effects.

4. *Variability*

The variability is greater when interference is present than under performance without interference. Table XIII. gives the medians of the average deviations in the card-sorting experiment for the men and women.

TABLE XIII

	Practice Groups		Group II		Group III		Group IV	
	Med.	P.E.	Med.	P.E.	Med.	P.E.	Med.	P.E.
Men	4	1	8	2	7	1	9	1.5
Women ...	7	1.5	4	1	9	1	5	1

In Groups II. and IV. there are only three subjects of each sex. In each group one of the men was very much slower, while the women happened to be quite uniform. So the figures for those two groups can not be considered of great value. In Group III. however there were seven subjects and consequently the results are much more reliable. With the exception of the women in Groups II. and IV., each of the interference groups show higher variability than the practise group.

When we turn to the percentage rates of improvement the variability becomes more significant. In the typewriting experiment the variability as expresed by the Pearson formula[7] before and after the break is as follows:

Regular men	.47 before break	.99 after break
Preliminary men	.37 before break	1.59 after break
Women	.58 before break	.93 after break

[7] Pearson formula, $\frac{\text{Gross variability}}{\sqrt{\text{Average}}}$. This is the form as modified by Professor E. L. Thorndike. See "Theory of Mental and Social Measurements," 1904, p. 102.

Nor do these rates of improvement correlate, showing that those improving fastest before the break do not improve most after it. The correlation figures are: Men, preliminary group —.86 P.E. .06, men regular group .13 P.E. .19; women .45 P.E. .17.

This variability is further emphasized by the fact that those making the most relapses have the slowest rate of improvement, as shown by the following correlations.

Between Relapses and Rate of Improvement

Men, regular group	$r = -.76$	P.E. 10
Men, preliminary group	$r = -.69$	P.E. 12
Women	$r = -.09$	P.E. 28

In the card-sorting the same thing is true. Table XIV. gives the variability as expressed by the Pearson formula for each of the groups, for both men and women.

TABLE XIV

	Practice Group	Group II	Group III	Group IV
Men, A_1	.83	.75	.55	.67
Men, A_2			1.20	.33
Women, A_1	.29	.83	.57	.34
Women, A_2			.80	.34

One subject among the men of the practise group brings the rate to higher than normal—Wa, who is the only subject of the entire series to begin as poorest and end up as best of the group. His first records were so phenomenally high as to make it purely accidental in that group. The rest of the table uniformly shows greater variability under interference.

In the discussion under plasticity the figures are given to show greater differences in plasticity under interference than in the simple practise curve, and consequently higher correlation between initial efficiency and final efficiency and between initial efficiency and rate of improvement.

CHAPTER V

Discussion and Summary

1. *Method*

Münsterberg[1] pointed out three experimental conditions for the investigation of his problem. They were: (*a*) The movements must be entirely mechanical so as not to call in the attention. (*b*) They must be easily varied. (*c*) They must call in the attention whenever a false movement is made or whenever the old habit returns. Because of the first requirement he held that the experiments could not be performed in the laboratory. The card-sorting experiment, it seems to the writer, meets all the conditions for the investigation of the problem of interference and meets them more decisively than his experiments do. It meets the needs of an experiment better because it can be more strictly measured, because it brings out greater possibilities of interference, and because it requires the maximum of attention.

In this experiment we have exactly similar conditions for each individual with reference to the number of repetitions, which would not be possible in an experiment of the kind described by Münsterberg. It means more exact measurement and makes possible a study of individual differences. In the second place the changes, coming quickly as they do, and requiring the exclusion of the ten former associations, give greater opportunity for interference to show its full force. Not until we know the full force of the interference are we ready to get any results of the overcoming of that interference. When a watch is changed to another pocket and the number of wrong reactions counted, there are a number of things which might affect the number of wrong reactions. The pressure of the watch in the other pocket, the accidental moves to that pocket, a lack of record of the number of times the watch was actually sought during the day, the varying conditions from day to day, leave a possibility that interference does not get its full play or at least gets a varying opportunity. By the immediate change of arrangements and by an equal number of repetitions in a given length of time this factor is better controlled.

This experiment requires the maximum of attention. The cards

[1] Hugo Münsterberg, *loc. cit.*, p. 71.

do not come in a given order and consequently there is no chance to grow lazy on the habit. There must be eighty individual discriminations and as many reactions. Attention did not waver in the card-sorting experiment as it sometimes did in the typewriting experiment. It continually calls the discriminative powers into play, but in the most simple form, a simple stimulus and its consequent reaction. The stimulus is not given until the card just above the one in question is thrown, and consequently the conditions for each are exactly the same. The point for which Münsterberg contended was that attention could not be centered on the performance before it was to take place. That condition is met here because the stimulus is not perceived until it must be acted upon.

The card-sorting experiment, it seems to the writer, is somewhat better than the typewriting experiment for the problems here investigated. The typewriting experiment, while perfectly valid, has two hindrances. In the first place there is such a difference between individuals in skill in manipulating a machine as to minimize the individual differences of the experiment. In the present experiment this factor has not been of such great importance because the results show that there is no correlation between the native skill on the typewriter and the ability to break the habit.

In the second place the gain on the typewriter is so great because of the poor initial performance, which in turn is caused by the fact that the subjects are awkward on the machine. This high initial record makes the retardation after the break seem small. After the break the subjects are familiar with the machine and can make fairly good records as compared with their initial records. The interference is clearly shown but not in the proportion which it really holds. Only in a few cases was the first performance after the break absolutely poorer than the first performance in the practise curve before the break. On the other hand in the card-sorting experiments the interference of the first four or eight sortings always made the time of the first sorting of the new arrangement longer. But there was no such great general transfer effect as in the typewriting experiment. If all the subjects were used to handling a machine the same thing would undoubtedly be true in the typewriting experiment.

The criticism has been made that the cards did not come in a given order. It has been held that the cards should be arranged in a given order for each ten, for example, 9, 1, 7, 2, 10, 6, 4, 8, 3, 5 for the first ten and then the same order repeated for every ten through the eighty cards. The subject could then learn this order and the arrangement and of course reach a better absolute time record than

in the present experiment. This would miss the purpose of the experiment. A chance order with no card repeating itself successively is the ideal for the purpose. Under the proposed scheme one would substitute units of ten for the single units. We have in the eighty cards just eighty repetitions of what would happen if a single stimulus appeared in a tachistoscope and a definite reaction was made, because each single card is seen only after the one above it is removed, and the entire process occupies in the later stages about three fourths of a second and with some was reduced to nearly one half second per card. The total time gives the measurement of eighty such simple reactions, each one under exactly the same conditions. Sorting the cards in sixty seconds means that it is practically automatic, or three fourths of a second is the automatic limit. If a change is then made to the other arrangement and the total time goes up to 80 seconds and one second is the measure of a single reaction. If in the second sorting the time is again sixty seconds we have a measure of the interference and the rapidity with which it is overcome both for the total and for a single reaction. The eighty cards thus arranged leave little room for error, which would not be the case with a single reaction.

Bergström, Bair, and Beazley used pictures on their cards, and Beazley used pictures on the keys of his instrument. The pictures which they used seemed not to be of equal difficulty nor can they be of equal familiarity. Bergström even used cards with a different picture on the other end. This it seems to the writer can not help being a disturbing element. Cards with numerals or letters or some other such simple stimuli should be used.

Bergström[2] allowed an interval of two minutes between the alternate sortings, or nearly two minutes depending on the length of time the sorting took. He had previously found that about two thirds of the decrease in the amount of interference took place in the first minute. How long an interval should be given between the sortings is therefore a very important factor. That he should find the interference effect so great after an interval, when according to his own experiments more than two thirds of the interference had already faded, is surprising and is discussed at another place. In the present experiment the intervals were kept uniform at thirty seconds. This, it was felt, would bring out the interference more strongly. The results here obtained, so different from Bergström's, were consequently not due to less rigorous conditions but to more rigorous.

The results of the card-sorting experiment show that for purposes of further investigation the alternate method and the four-by-

[2] Bergström, *loc. cit.*, p. 436.

four trial method of Group III. are the best. Nothing new is yielded by the eight arrangement method except that the interference is not much more, if any, with eight than with four repetitions, and that it is sooner overcome. The four-trial method seems to be the best employed by the writer.

2. *Relation of Interference to Practise Effect*

The specific problem with which Bergström was concerned in his second report on interference effect was its relation to the practise effect, as to whether it was greater, equal to, or less than the practise effect. His conclusion was that the interference effect was equal to the practise effect.[3] He also says that if the experiment had been taken at the same stage of practise as the one he previously performed the interference effect might have been greater than the practise effect. He thinks that the previous and opposing associations are not effaced because they have the same effect on a third arrangement as they have on each other. Bair finds no such large amount of interference and thinks a large part of Bergström's is due to indisposition on the part of the subject.[4]

Bergström had only one arrangement, that of alternating, and conducted it for just eight repetitions, which is included in one day of my experiment. He might use the same cards after several weeks for another similar series but was sure that at that time no practise effect remained over. Then in the second place he used only one subject. With only one subject for such a short time he could hardly draw his strong conclusion, for several of my subjects did not show improvement until the second day, and had they been all used for just one day there would have been little improvement.

All of the twenty-six subjects in the present experiment show interference effect. Immediately they show more than Bergström's for the sortings were only 30 seconds apart. This is not a matter of indisposition or laziness but interference. The hardest work of the subjects was done right after the change and just when the interference showed most. Except when a record was to be broken the subjects worked hard to keep their time from increasing. Only one subject did not do this and he showed laxness of interest at several points. In spite of hardest work at such times every individual shows immediate interference, and those showing most improvement often show greatest immediate interference.

But the interference is overcome and the two associations become

[3] Bergström, *loc. cit.*, pp. 440, 441.

[4] Bair, *loc. cit.*, p. 47.

automatic. The interference bears a decreasing ratio to the practise effect. Bergström assumes that, if the interference and practise effects are directly proportional to the number of repetitions, the one will be equal to the other times a certain constant. If this constant is equal to 1, interference is equal to the practise, if greater or less than 1, the interference is greater or less than the practise. If they are not directly proportional to the repetitions and yet hold the same relation to each other, then both would have the form of the true practise curve, or nearly so at least, as the mechanical conditions of the experiment admit. However, since the curves of the two opposing associations follow the parallel of the abscissas he holds that the interference and practise effects do vary directly with the number of repetitions and hold the constant relation of one to one to each other and are therefore equal.

The curves of the present experiment show that in simple practise the practise effect varies with the number of repetitions though not directly proportional for the curve is concave. The question then arises whether the interference varies in the same way. If it does its relation to the practise effect would be constant. The constant quantity would be less than 1 if the curve having interference shows any practise effect.

But in the present experiment the interference effect does not vary with the number of repetitions in the same way as does the practise effect. Bergström showed that after twenty-four hours there is no trace of interference left, and the present experiments indicate no interference from the previous day. The practise of one day however begins with the strength of the association of the previous day. Thus on the first day alone would the interference effect and practise effect be based upon the same number of repetitions. On the succeeding days the interference effect would be based only upon the immediately preceding opposing repetitions while the practise effect would be based on all the repetitions of the entire experiment. There can then be no constant relation of the interference effect to the practise, but a variable and continually decreasing relation.

That there is a variable and decreasing relation the curves of the alternate group most clearly show. The practise curve is concave, the alternate group almost a straight line but continually improving to the end. The interference is therefore continually becoming less as related to the practise and if the experiment were carried on for a very long time the two would undoubtedly come together. There is temporary disadvantage to be overcome, which would make just a little difference but would become infinitely small. The interfer-

ence then seems to hold a variable relation to the practise, being just less than 1 at the beginning and approaching 0.

The curve of Group IV. is just as concave as that of the practise group. The interference is readily overcome within the periods themselves and new practise effect is continually gained as if the interference were not present. It is simply temporary within each change of arrangements.

In Group III. the curve shows slightly less improvement than that of the practise group, but it is nevertheless concave and continually reaches the practise group. An indefinite amount of practise would bring the curves closer together and the immediate interference of the change would show less and less.

That the interference is an incident of the course of two associations becoming automatic is clear, but it is overcome, and the two opposing associations become automatic in each of the three groups, with the advantage to those groups having the greater number of consecutive repetitions before changing to an equal number of the opposing reaction.

3. *Physiological Considerations*

The question has been raised as to the condition of the neural paths of the previous association when the new association becomes automatic. Münsterberg held that physiologically there remains a molecular disposition of the paths used so that they are more easily reopened when returned to than in the first practise.[5] Bergström asks whether the old association is effaced after the practise of the new one.[6] He concludes that it is not because if a third arrangement A_3 is sorted it shows the interference of A_1 the same as A_2 does. He further holds that the interference effect of the A_1A_2 series is no less than that of the A_1B_1 series, which we should expect to be the case if the opposing associations had partially effaced each other.

What physiological explanation would then fit his conclusion that the interference effect is equal to the practise effect? If an association fixed by a certain number of repetitions has a well-worn pathway of discharge, that particular association ought to function more readily and automatically at the nth repetition than it did at the first, unless it is effaced. His conclusion seems to me contrary to all that we know of the neural process of habit formation. That it is not effaced seems true but then it should function automatically.

The results of the present experiment show just what the physiological facts would lead one to expect. The associations are not

[5] Münsterberg, *loc. cit.*, p. 71.

[6] Bergström, *loc. cit.*, p. 440.

effaced but continually become better worn and function more readily. The high rise in the curve of Group III. comes back to the practise level almost immediately, showing that the association is as strong as before the interference. The same thing is true to a greater degree in Group IV. In the alternate group the first sorting of each day is distinctly better than the others, showing that the association has become well fixed.

Fig. 2 is a schematic representation of the neural connections in

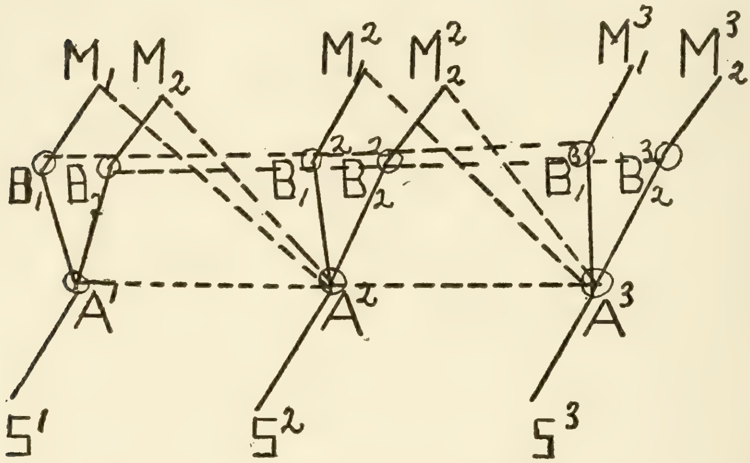

FIG. 2. S, S^2, $\cdots$ represent the individual sense impressions received from the various cards coming in to the sense centers A, A^2, $\cdots$. In the first arrangement these are discharged into the motor cells B_1, B_1^2, $\cdots$, so as to bring on the movements M_1, M_1^2, $\cdots$ throughout the first series. The sense impressions from the stimulus in the second arrangement are the same, so that from the same sense centers A, A^2, $\cdots$ there are again discharged impulses, but these pass into motor cells B_2, B_2^2, $\cdots$, which bring about the movements required in the second arrangement, represented by M_2, M_2^2, $\cdots$ throughout the entire series. According to the law of contiguity connections are worn between A^1, A^2, $\cdots$, as shown by the dotted line. Likewise connections are worn between B_1, B_1^2, $\cdots$ and between B_2, B_2^2, $\cdots$. There are also connections formed between the sensations of the movements and the original sense center as shown by the dotted lines. We have established connections between the sense center and two opposing motor cells and consequent movements. Both, however, become automatic and when S is started each will follow according to the law of habit. The movements will follow one arrangement or the other, according which is started and receives the temporary advantage.

the case of the card sorting. S represents the card stimuli pouring into the sense centers A. In the first arrangement there is a certain pathway worn S, A, B_1, M_1, and so on for the whole ten cards. In the second arrangement we have the same stimulus, and the same

sense center effected but the discharge is through a different motor cell and a subsequent different movement, the whole path being S, A, B_2, M_2 and so on through the entire ten cards. There is another set of stimuli coming from the arrangements which are different in the two cases and the neural connections of these determine whether A will discharge into B_1 or B_2 and consequently give movement M_1 or M_2. But out of A there are these two pathways of discharge for the same stimulus and neither is effaced, but both are well worn. Now A, by the law of contiguity connects with A^2, A^3, . . ., B_1 connects with $B_1{}^2$, $B_1{}^3$, . . ., while B_2 connects likewise with $B_2{}^2$, $B_2{}^3$ and each of the movements M_1 and M_2 connect with A. So all the connections are formed as in forming a single habit, except that here we have two opposing habits and yet both of them becoming automatic. As to which direction the discharge will go depends on the larger complex in which this situation lies, but the fact is that both of them are open and ready to discharge. Münsterberg said that the discharge will follow either path and that it will not divide. This is the case in the experiments, and this the figure in a schematic manner attempts to show in the neural connections.

4. *Adaptability*

In the introduction it was stated that adaptation is the general character of mental development, that adaptative reconstruction is the general form of change which is incessantly renewed so long as the individual continues to live, and that this embodies an elaborated form of response and a modification of the response to meet a change in the system of stimuli. The question of adaptability was found to have wide practical interests, racial, political, religious, social and industrial. To investigate these in their broad relations is impossible under experimental conditions. Observation shows that in all these fields adjustments are made with greater or less ease among individuals and groups. The friction which opposes these adjustments or in a more general sense adaptation, is interference. Interference seems to be present to a greater or less extent among all such adjustments.

For the making of these adjustments the nervous system has groups of inherited tendencies, the simple elements of which are associative connections. With these associative connections the experiment has had to do. If these larger complexes have adjustment properties we should expect the associative elements to have them also, though not to such a great extent. If an individual can adapt himself in these wider fields of interest and activity he should be

able to do the same to a lesser degree in the simple associations. What is true of life should be found true in the laboratory.

The experiments show that on simple association interference is an *incident,* but only an incident and not fundamental. Adaptation is the fundamental thing, because the two associations do become automatic. In the wider interests interference is an incident but adaptation is fundamental. This is true in religious conversion, in industrial changes, in social readjustments, in racial migrations and in every form of life where variations occur in the environment. That it is true in associaton *per se* preserves a unity in the nervous organism.

Bergström says in speaking of these wider applications of the problem:[7] "The nervous system has, as is well known, inherited tendencies of growth and adjustment to external circumstances. Perhaps the simplest of these for the organization of nervous activity is the tendency of nerve currents to run from one pattern to the next succeeding. This is modified in many ways by special tendencies of a higher order, which may be classed as fundamental practical adjustments or practical interests. If these are given opportunity to influence the results, we are not dealing with associations *per se,* but with these as modified by other more powerful forces. . . . There are besides great numbers of secondary tendencies by which simple successive association is transformed. Moreover where the conditions are more complex than in this experiment certain conditions may enter and change the results. A person who speaks several languages finds that the words of the same language tend to be recalled together. Some persons also know and can use two different systems of shorthand. In these cases the elements associated are used as members of a group, for the exclusive employment of which there seems to be a strong tendency. The inconvenience of interference is thus to some extent avoided in these cases, but probably only by a proportionate expenditure of energy."

In other words the elements of the complexes show an eternal interference to the tendencies of the complexes themselves. It is rather peculiar that an individual can use automatically two systems of shorthand, each simple association of which excludes the opposing one, and yet can never learn to shuffle a few cards when their associations exclude each other as Bergström's discussion indicates. That there are these higher complexes and that they show wider adaptive tendencies is freely admitted, on the principle that the higher the process the wider the differences and the greater the

[7] Bergström, *loc. cit.,* p. 442.

plasticity. But that there is an irreconcilable conflict is denied. That the larger adjustments are made only because the superior energy overcomes the inferior associative element does not give us a unity but gives us an unexplained difference. If this were the case then no investigations of association would have the slightest value in practical interest and the results of experimental psychology would be in endless contradiction with the truths obtained by observation and general experience. Though we are not yet able to show the correlation between results on simple association and these higher adjustments the search is not futile, but it would be hopeless if the results on association *per se* conflicted irreconcilably with these tendencies which are only groups of association elements, and their adjustment had to be explained on the ground that the lower elements are overcome by their greater energy.

The individual differences in adaptability are great and this is what general experience confirms. The discussion of plasticity as a section in ''Individual Differences'' shows a correlation between the initial efficiency and the improvement under conditions of interference. This correlation is higher in the groups having interference than in the practise group. The more complex the situation is and the more it calls for adaptability and adjustment in the face of interference, the more does the individual ability appear. Greater ability of performance at the start indicates greater power of adaptability. The fittest will adapt best and survive, the ones most accustomed to the one situation will most readily adapt themselves to the new. The more fully one set of habits is built up the better fitted is the organism for having many habits with mutually exclusive associations.

According to Bryan and Harter the higher the organism the more complex the hierarchy of habits.[8] The higher the organism the greater the power of adaptability. The experiments show that there is progress in spite of interference because fully developed habits are formed, and the more completely each of these habits are formed the less influence interference has and the greater the adaptability of the individual. Adaptability is not manifest because there is little interference from a weakly opposing association, but because all the associations are strong, and the interference has no power of effacing the opposing associations. While Bryan and Harter discuss a different problem in their hierarchy of habits, yet the same general conditions of adaptability seem to underlie the ready formation of a hierarchy of habits, and the formation of a complex of

[8] Bryan and Harter, ''On Learning the Telegraphic Language,'' *Psych. Rev.*, **6**, 1899.

habits as in the present experiment. Their contention also that the hierarchy of habits is an index of higher type of organism agrees with the building in of habits of the present experiment.

The discussion of ''variability'' also brings out the fact that individual differences are greater under conditions of interference than in the simple practise groups. The average deviation is greater in Group III. than in the practise group. The variation in the rates of improvement as expressed by the Pearson formula are greater after the ''break'' than before in all the groups of the typewriter experiment, and also among the groups of the card-sorting experiment the variability of the groups under interference is greater than that of the practise groups. The sex differences are not so pronounced.

Ethical Implications

Professor James has pointed out the enormous consequences of habit as being the precious conservative agent of society.[9] The plaster of life is early set and forever thereafter we are content to remain in the mold. Men grown old in prison ask to be readmitted. While this is true there is another side to the truth. Some in whom the mold has long been set move from the foreign land to this and enjoy the remainder of their days. There are limits to which the plaster will reset to the new conditions. There are adjustments made long after the old associations had been firmly fixed. After the change the new forms for itself another mold of habit and if the former is not returned to will be just as well set as if it were the only one. If the former is returned to the individual will be as perfectly adapted to either as he once was to the first.

One of Professor James's oft-quoted maxims is ''Never suffer an exception to occur until the new habit is securely rooted in your life.'' How true this is many of the subjects in the typewriting experiment can fully testify. Often when the new order seemed to be well established and the old troubled them little, one relapse would set them wrong for the rest of the experiment and it would be harder for them to overcome it than after the immediate ''break.''

There is another truth which seems to the writer to have great importance and value. Do not passively resist the temptation of the old stimulus, but respond to it by another action which will exclude the previous action. Let the reformed drunkard resist the temptation to drink by every time taking some little refreshment which will give a definite response to the stimulation, but which will exclude the former. Religious societies have long recognized in a general way that the new convert must have other work to do to satisfy his

[9] William James, ''Principles of Psychology,'' Vol. 1, p. 121.

impulses. Only by action which will exclude the former association will there be real success in excluding the former and a true method, psychologically speaking, of ingraining the latter.

Summary

The results of the experiments may be summed up as follows:

1. When two opposing associations, each of which excludes the other, are alternately practised with one, four, or eight repetitions of each association before the other is resumed, the opposing associations have an interference effect upon each other in all the subjects. The interference effect grows less and less while the practise effect becomes greater. The interference effect is gradually overcome and both opposing associations become automatic so that either of them can be called up independently without the appearance of the other. The curves of the alternating group follow a straight but descending line and gradually approach the true practise curve. The curves in which four or eight repetitions of the one association are given before the other is resumed are concave and closely approach the practise curve. The individual differences in the rate of improvement are as great as in the absolute time records, but in no case is the interference effect equal to the practise effect. Adaptability is fundamental with individual differences; interference is an incident in the course of automatization of the two opposing associations.

2. When a change in reaction to several of a series of long practised stimuli is introduced, as in the typewriting experiment, there is great immediate interference effect. This is shown by the increase in time and the recurrences of the former associations. The improvement is rapid, the interference effect is overcome, and the previous level of efficiency attained. Individual differences are greater than before the change was introduced. The number of recurrences of the former association varies from 2 to 66. There are no sex differences, except that the women are more variable than the men.

3. There is a positive correlation between the adaptability of the eight subjects of the preliminary group of the typewriting experiment and the traits of *individuality, independence* and *originality.*

4. There is a positive correlation between adaptability as shown by the discrimination to red after long practised association with blue, and the color naming test of 100 colors.

5. An error committed in practise tends to introduce interfering associations which will cause other errors. In some cases this interference has a general effect which causes various errors; in other cases it has a specific effect which causes a repetition of the error in

succeeding trials. Some individuals are affected mostly in the first way, while others are affected chiefly in the second way. There are great individual differences in the interference caused by errors.

6. There is a high correlation between initial and final efficiency which shows that the more efficient improve as much as or more than the less efficient. This correlation is higher when interference is present than it is in simple practise, thus bringing out the individual ability more clearly. There is also a slight correlation between initial efficiency and rate of improvement in the groups having interfering associations, while there is none in either the simple practise group or the typewriting experiment.

7. There are no significant sex differences in rate of improvement. The men show greater immediate effect after the change from one sorting to another, but they very rapidly get back to their former level.

8. The variability is greater when interference is present than when it is not. This is shown in a comparison of the practise group with the others in the card-sorting experiment and also by comparing the rate of gain after the break with that before the break in the typewriting experiment. The women show greater variability in relapses but not in actual time records nor in rates of gain.

REACTION TO MULTIPLE STIMULI

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REACTION TO MULTIPLE STIMULI

I.

Introduction

The questions of reaction to simultaneous and to successive stimuli have received but little attention. They are important as they furnish more or less definite solutions to the older problems concerning the nature of the sensory-motor event in the various responses to stimuli, and suggest new problems for investigation. They also throw a side-light on the questions of the relation between intensities of stimuli, types of attention, and reaction-time.

The present investigation has four purposes: First, to study the time of reaction to simultaneous stimuli when presented in pairs and groups of three, with the reaction attention directed to a given member of a pair or group, and without. Second, to investigate the ability of a reagent to select from a pair or group of simultaneous stimuli one of its members for reaction, by comparing his reactions under this requirement with those in which he responds to the undifferentiated group. Third, to study the facilitating effect of stimuli of medium intensities upon the reaction-time to stimuli of low intensities; and, Fourth, to observe the effect upon the reaction-time to a light stimulus of presenting before it, at gradually reduced intervals, one or two other stimuli of different kinds. Each of these problems has other subordinate elements that are treated in connection with it.

The writer gratefully acknowledges the assistance of Professors J. McKeen Cattell and R. S. Woodworth, of Columbia University, in planning and carrying out this experiment, of Mr. A. T. Poffenberger, Jr., who was subject during the entire period of experimentation, and of Dr. M. E. Haggerty, director of the psychological laboratory of Indiana University, who has kindly read the proof.

II.

The Production of the Stimuli

The stimuli were presented by a heavy wheel (*W*, Fig. 1) 92 cm. in diameter, with a rim 17 cm. wide, mounted horizontally on a table and revolving on ball-bearings. The wheel was driven by a motor (*P*) by means of a belt which passed around its rim and ran without noise. This manner of driving the wheel was chosen in preference to the device used by Froeberg[1] who first employed the wheel for the presentation of visual stimuli. A constant motor was employed which ran always at its maximum velocity causing the wheel to make one revolution every 1.25 sec.

§ 1. *The Light Stimulus.*—A small 2 c.p.-4 v. incandescent bulb in a black box (*L*, Fig. 1) is mounted upon the rim of the large wheel and above the motor belt. The light is emitted as a clean white light after passing through a ground glass and a blue glass slide. The current is supplied to the incandescent bulb as it is carried around by the revolving wheel by the following arrangement. Starting from one of the poles of the rheostat (Rt_1) the current is led to a binding post (l) in the base (B) which supports the large wheel, and is there grounded. The entire wheel, except what has been carefully insulated, is thus potentially charged with electricity from the initial pole. The current is then taken up at the binding post (l_1) on the rim of the wheel, and near the lamp (L) and wired to one of the poles of the lamp, thence from the other pole to the post (l_2), carefully insulated from the wheel. The current then passes a copper brush to a small brass wheel (w) on the spindle of the large revolving wheel and carefully insulated therefrom. From a binding post (l_3) which the wheel bears the current is conducted to a mercury cup (M) bored from wood fiber for the purpose of insulation and driven into the end of the spindle of the large wheel. From the mercury cup the current is taken up by a hooked rod (l_4) and conducted to the stimulus key (K_1) and thence to the other pole of the rheostat (Rt_1). This describes the complete circuit of the light current.

As the experiments were conducted in a room lighted by diffuse daylight care was taken to keep the intensity of the light constant. It has been shown that a variation in the intensity of the light

[1] "The Relation between the Magnitude of the Stimulus and the Time of Reaction," Archives of Psychology, No. 8, 6, 1907.

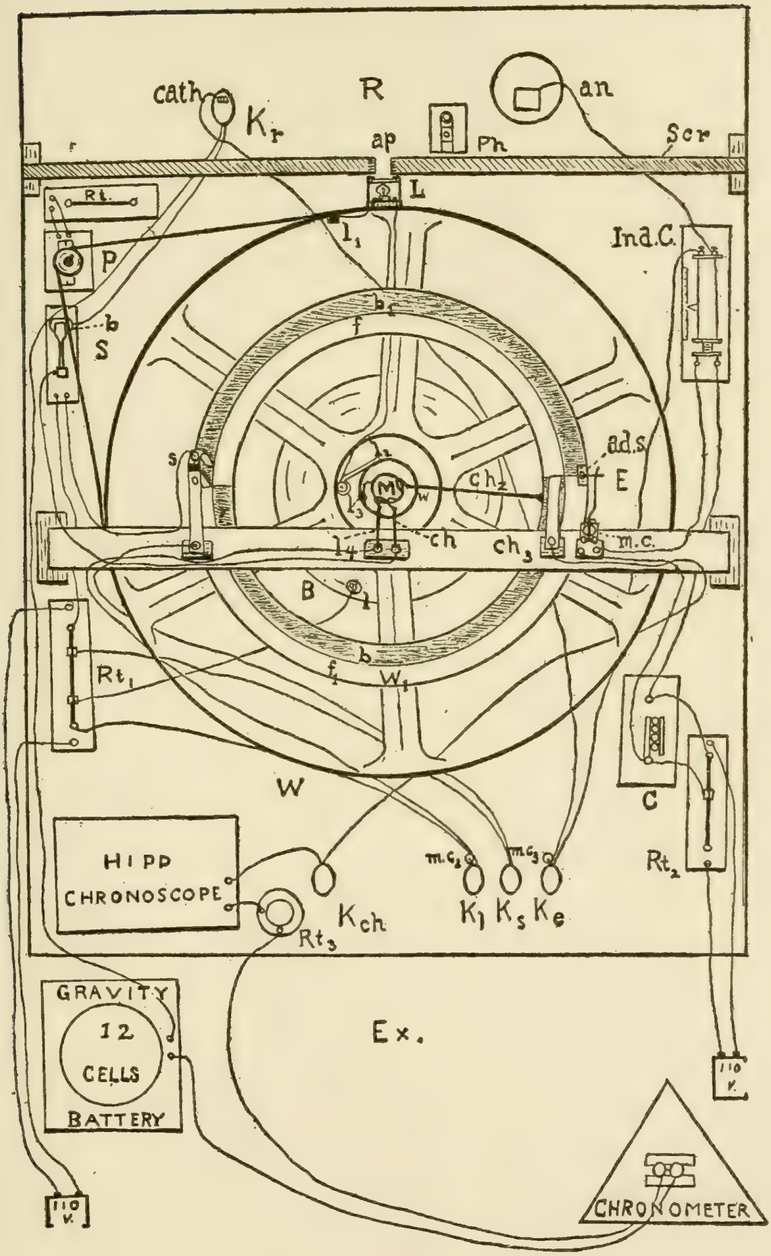

FIG. 1. Stimulus Apparatus.

stimulus produces a variation in the reaction-time.[2] Cattell,[3] however, has pointed out that the influence of a difference in intensity is not very great except in the case of very weak stimuli. While with one exception,[4] the light stimuli employed in this experiment were in all cases of what for convenience may be called medium intensities, a photometer was used to detect changes in the intensity of the light in the experimental room. The photometer (*Ph*) was placed directly beside the aperture (*ap*) in a tall screen through which the reagent received the stimulus. The curtains of the room were either raised or drawn as the photometer indicated. On very dark days it was impossible to reproduce the usual light intensity and experimentation was abandoned, as it was thought that small variations might tend to conceal differences which the use of the stimuli might otherwise show.

§ 2. *The Electric Shock.*—The shock was produced by the sudden break of the primary circuit of a small induction coil. Several and unsatisfactory devices were tried for breaking the primary circuit, among them a spring contact that was abandoned because its rebound produced a sound that disturbed the reagent. A wheel contact was tried and abandoned as it was necessary in this experiment to have the device self-setting after having been turned to break the circuit, and this could not be done without the use of a spring. Upon resetting the spring caused the wheel to rebound several times producing a number of rapid successive shocks after the first breaking of the circuit. These shocks, while not occurring till a few sigmas after his reaction, disturbed the reagent and interfered with his next reaction. The contact finally adopted (*E*) consists of a short lever of the second class connected with one of the binding posts of the device. Near the middle of the lever is a metallic point that dips into a mercury cup (*m.c.*) which with the binding post already mentioned constitute the two poles of the device. The large wheel driven by the motor carries with it a smaller concentric wheel (W_1) upon which is clamped an adjustable slide (*ad.s.*). This slide carries an adjustable rod carefully set so as to lift the lever high

[2] Exner, "Experimentelle Untersuchungen, etc.," *Pflüg. Arch.*, **7**, 625, 1873. Also, "Physiologie der Grosshirnrinde," *Hermann's Handb.*, **2**, 269, 1879. Berger, "Über den Einfluss der Reizstärke auf die Dauer einfacher psychischer Vorgänge mit besonderer Rücksicht auf Lichtreize." *Phil. Stud.*, **3**, 38, 1886. Cattell, "Influence of the Intensity of the Stimulus on the Length of the Reaction-time," *Brain*, **32**, 512, 1886. Froeberg, "The Relation between the Magnitude of the Stimulus and the Time of Reaction," ARCHIVES OF PSYCHOLOGY, **8**, 1907.

[3] "On Reaction-times and the Velocity of the Nervous Impulse," *National Academy of Sciences*, Second Memoir, **7**, 394, 1893.

[4] See division VII. of this paper: Reaction to Stimuli of Low Intensities Accompanied by Simultaneous Stimuli of Medium Intensities.

enough, as the wheel revolves, to raise the metallic point out of the mercury in the cup and thus break the primary circuit. The lever's gravity closes the circuit again after it is broken. This device is noiseless as the plane of the lever is at about an angle of 25° with respect to the horizontal plane of the adjustable rod and traverses about one half the lever's length before breaking the circuit, insuring thus a glancing blow that is no more vigorous than a sudden lift of the lever. The velocity of the rod is about two meters per second. So as to readily break the surface tension of the mercury and to minimize its tendency to draw up with the metal point, the latter is sharpened and thus offers the mercury less surface. The lever fell back upon a felt cushion which eliminated any noise due to its drop.

A condenser or accumulator (*C*) was introduced across the primary circuit to minimize—it is impossible to eliminate entirely—the spark produced at (*E*) upon each break of the primary circuit. It was thought that this spark delayed the true breaking of the circuit from five to six sigma.[5] It is well to remark that unless the induction coil has a laminated core it will not respond to the effect of the condenser which can work only where rapid demagnetization of the core is possible. When the condenser is used the induced current is shorter and more intense than is otherwise the case. The negative electrode (*an*) of the secondary circuit is deposited in a vessel of fairly saturated saline solution to the left of the reagent's position (*R*). The positive electrode (*cath*) is extended to the reagent's key (K_r) and is so arranged as to necessitate the induced current's passing through the experimenter's key (K_e) which gives the electric stimuli to the reagent. This key is provided with a mercury cup ($m.c.^3$), which insures against the change of intensity due to oxidation found in a dry contact key. To receive the electric shock stimulus the subject places his left hand in the saline solution (*an*) and his right hand upon the cathode in the reaction key (K_r). Placing the positive pole of the induction coil in the reaction key prevents the subject from feeling two shocks of about the same intensity—one upon the breaking and another immediately afterward upon the re-making of the circuit—as would be the case if

[5] The delay is not to be thought of as due to any latency in the production of the induced current by the breaking of the primary circuit. Physicists have determined that when an induction coil with a laminated core together with a condenser is used the induced current is to all practical purposes instantaneous. Cattell and Dolley (National Academy of Sciences, Second Memoir, 7, 402, 1893) tested an induction coil for latent period of induced current by means of a chronograph and found no appreciable interval between the breaking of the primary circuit and the induced spark.

the negative pole were in the reaction key. This arrangement causes the stimulus to be received through the reacting finger. It has been shown that such arrangement produces a shortening of the reaction-time.[6] Exner,[7] however, has found the reaction-time to be as much as 10^{σ} longer when the stimulus was received by the reacting hand than when it was received by the other.

§ 3. *The Sound Stimulus.*—The sound used in this experiment was produced by the more or less familiar electro-magnet sound hammer figured and described elsewhere.[8] A circular strip (W') clamped to the upper side of the large wheel (W), and lying in the plane of the wheel carries in its surface two smaller circular strips, each half of brass (bb') and half of thin wood-fiber (ff_1). The outer brass strip (b_1) is connected in no manner with the current, but serves to complete the circuit at the proper time through the brush (s) that presses upon it. This brush consists of two smaller brushes a few centimeters apart and insulated from each other by a small block of wood-fiber, shown in Fig. 1 by a black band between them. Each of these smaller brushes is connected with one of the poles of the rheostat (Rt_1). As the large wheel revolves the brushes pass alternately upon the brass strip and upon the wood fiber. The sound circuit is broken when the brush is on the fiber but is made when it is on the brass by the current's passing from one brush to the other through the medium of the intervening brass. The armature of the electro-magnet sound hammer is held down until the brush again passes off the strip and upon the wood-fiber. Reckoning from the velocity of the wheel this was for a period of 625^{σ}, or much in excess of the longest interval employed in the experiment on successive stimuli explained later in this paper. It is necessary that the sound hammer be held down as it completes the chronoscope circuit. So that the experimenter could readily open and close the hammer according as sound was or was not to be one of the stimuli in a given course of the experiment, it was necessary to place it at a distance of about three feet from the reagent. This caused the stimulus to reach his ear 3^{σ} after the chronoscope had started. This error was not taken into account at first but was later corrected for.[9]

[6] Reigart and Sanford, "On Reaction-times when the Stimulus is Applied to the Reacting Hand," *Am. J. P.*, **4**, 351, 1892. Cattell and Dolley, "On Reaction-times and the Velocity of the Nervous Impulse," *National Academy of Sciences,* Second Memoir, **7**, 410, 1893.

[7] "Experimentelle Untersuchungen der einfachsten psychischen Processe," *Pflüg. Arch.*, **7**, 622, 1873. See also Exner's table in Hermann's *Handb. d. Physiol.*, **2**, Pt. 1, 264, 1879.

[8] Titchener, *Exper. Psych., Stud. Man.* (Quant.), 154, 1905. Myers, *Text bk. of Exper. Psychol.* (sec. ed.), II., 43, 1911.

[9] See division IV. of this paper.

The intensity of the sound was kept practically constant by the use of a mark on the millimeter scale beside the hammer which insured in each case that the hammer fell the same distance at each stroke upon the solid base. It has been shown by several investigators[10] that a difference in the intensity of a sound stimulus produces a difference in the reaction-time, although as Cattell[11] points out a difference in intensity does not have a very great influence except in the case of very weak stimuli. Two observers[12] have reported experiments which resulted in their finding no effect upon reaction-time of change in the intensity of the sound stimulus. It is altogether likely that these experiments were not elaborate enough to warrant the conclusions. In the present experiment it was found that a reduction of the distance through which the sound hammer fell from 12 mm. to 2 mm. caused an increase of 22.4^{σ} in reagent *T*'s reaction-time and an increase of 26.1^{σ} for reagent *P*. These increments are obtained from 100 reactions for each subject to the two different intensities of sound. Reactions to two lights and two electric shocks were likewise obtained and differences of 19^{σ} and 24^{σ}, 24^{σ} and 5^{σ}, respectively, were obtained.

§ 4. *The Method of Measuring the Reaction-time.*—The Hipp chronoscope used in this experiment is the same instrument that was employed by Cattell and Dolley[13] and has several improvements made by these investigators which render it much more reliable than the chronoscope ordinarily used in laboratories. As the value of the chronoscope consists in the application of the electro-magnets, they rewound the electro-magnets with coarser wire, which greatly reduces the latent period of their magnetization and demagnetization and allows the chronoscope to record more accurately the true reaction-times. This also obviates the use of a commutator in this experiment. Külpe and Kirchmann[14] proved that the Hipp is not influenced by the length of time the current runs through the

[10] Wundt, ''Veränderungen d. einfachen Reactionsvorgangs durch äusz. u. innere Einflüsse,'' *Physiol. Psych.*, 351, 1887. Berger, ''Über den Einfluss der Reizstärke auf die Dauer einfacher psychischer Vorgänge, etc.,'' *Philos. Stud.*, **3**, 64, 1886. Froeberg, ''The Relation between the Magnitude of Stimulus and the Time of Reaction,'' ARCHIVES OF PSYCHOLOGY, **8**, 33, 1907.

[11] ''On Reaction-times and the Velocity of the Nervous Impulse,'' *National Academy of Sciences*, **7**, 394, 1893.

[12] Martius, ''Uber den Einfluss der Intensität der Reize auf Reactionzeit der Klänge,'' *Phil. Stud.*, **7**, 469, 1891. Slattery, ''On the Relation of the Reaction-time to Variations in Intensity and Pitch,'' *Stud. fr. Yale Psych. Lab.*, 71, 1892.

[13] ''On Reaction-times and the Velocity of the Nervous Impulse,'' *National Academy of Sciences*, Second Memoir, **7**, 395 f., 1893.

[14] ''Ein neuer Apparat zur Controle zeitmessender Instrumente,'' *Phil. Stud.*, **8**, 153 f., 1893.

magnets during so short a period as that of the simple reaction period. They found also[15] that the error of the chronoscope remained constant for readings between 56 and 598σ. Berger[16] found the error to be constant for times between 200 and 400σ. Again, Müller and Schumann[17] found the error constant for long intervals. Edgell and Symes[18] conclude from their tests that when the errors due to overtones in the vibrating reed are eliminated by proper adjustment of the escapement, the error of the chronoscope is constant for readings of great length. They state that the current should "be frequently and regularly reversed."[19] Dunlap[20] does not think reversing the current is necessary: "The reversal of the current through the chronoscope magnets is clearly useless, and is apt to introduce errors of appreciable amount, since the experimenter is certain to forget frequently to reverse the commutators."

To further insure the reliability of the chronoscope we employed the Cattell gravity chronometer elsewhere figured and described.[21] This control instrument, on the principle of the Atwood machine, allows adjustment for different standard time intervals. A heavy weight falls the length of a 2 m. column starting and stopping the chronoscope by means of two wheel contacts set for a standard interval. Checking the chronoscopic reading with the standard interval shows its reliability. Before each sitting ten control readings were taken. The current was adjusted till the chronoscope showed an average variation of at the greatest no more than 1σ. Cattell[22] found that the gravity chronometer gave, in a test of three successive series of ten trials each, with a normal time of 100σ, readings whose means variations were 0.54, 0.64 and 0.56.σ

In 1910 while we were devising the present apparatus we made tests upon the reliability of two chronometers and tests of two sources of current. We used the simple form of the control hammer[23] and a portable gravity chronometer, but could not

[15] *Op. cit.*, 170 f.

[16] "Über den Einfluss der Reizstärke auf die Dauer einfacher psychischer Vorgänge," *Phil. Stud.*, **3**, 93, 1886.

[17] In Müller and Pilzecker, "Exper. Beiträge z. Lehre von Gedächtniss," *Zeit. f. Psychol.*, Ergänzungsbd. **1**, 292 f., 1900.

[18] "The Wheatstone-Hipp Chronoscope. Its Adjustments, Accuracy and Control," *Brit. J. of Psychol.*, **2**, 85, 1906.

[19] *Op. cit.*, 86.

[20] "The Fall-hammer, Chronoscope and Chronograph," *Brit. Jour. of Psychol.*, **4**, 55, 1911.

[21] Cattell and Dolley, "On Reaction-times and the Velocity of the Nervous Impulse," *National Academy of Sciences*, Second Memoir, **7**, 397, 1893.

[22] "Chronoscop und Chronograph," *Phil. Stud.*, **9**, 309, 1894.

[23] Myers, "Text-bk. of Exper. Psych.," Pt. II., 49, 1911. Titchener, *Exper. Psych., Stud. Man.* (Quant.), 152, 1905.

properly control the chronoscope with them. The control hammer, however, was used with a motor-dynamo current, and not with the gravity battery with which the reaction times reported in this experiment were obtained. But as it showed a greater variation than the large gravity chronometer when used with the same current, it is certain that we could not have safely used it. In another experiment going on in the laboratory at the same time as the one here reported, we tested another Hipp chronoscope with the portable gravity chronometer, using in one case the current from the university power-house and in another the motor-dynamo current, and found in the first case variations as high as 15^σ and in the second variations a very little shorter. These results were valuable for comparative purposes only, as we had early determined to use a battery of 12 gravity cells. Much of the blame that is placed upon the Hipp chronoscope is more properly referable to the use of an unsteady current, the retention of the electro-magnets with which the chronoscope is equipped, and to a faulty control apparatus.

The Course of the Current through the Chronoscope and the Stimulus Apparatus (Fig. 1).—Starting at one pole of the gravity battery the current passes directly to the reaction key (K_r); thence to the sound hammer (S); through the metallic base (b) and the hammer; to a binding post in the cross-piece at the spindle of the wheel and into the mercury cup (M) in the end of the spindle by means of the rod (ch) from which the rod (l_4) of the light circuit is carefully insulated; thence by means of another rod (ch_2) out to the brass strip (b) to which it is soldered. As the large wheel revolves the chronometric contact brush (ch_3) passes alternately upon the brass strip (b) and the wood-fiber strip (f), alternately closing and breaking the chronoscope circuit when the other parts of the line are closed. From the brush (ch_3) the current runs directly to the key at the experimenter's left, which key is closed prior to the giving of the stimulus. Beginning at the other pole of the battery the current may be traced through the gravity chronometer and to a small rheostat (Rt_3), which affords adjustments when the control readings are being taken, and thence to the other pole of the chronoscope. As the wheel makes one revolution in about one and one fourth seconds, the brass strip (b) whose length is one half a circumference, keeps the chronoscope circuit closed for about 625^σ. It is estimated that out of every experimental period of from 40 min. to 1 hr. in length, while the experimenter was recording the times and giving the signals, the chronoscope circuit was open two thirds of the time.

III.

Reaction to Simultaneous Stimuli

§ 1. *Arrangement of the Apparatus.*—In this division the stimuli were presented as follows: (*a*) Singly; (*b*) in pairs, with instruction to react to one of the members of the pair; and (*c*) in groups, with instructions to react to a given member of the group. After these arrangements had been investigated the stimuli were presented in the following manner: (*d*) Singly but of low intensities; (*e*) in pairs, one member of which was of low intensity while the other was of the intensities of the stimuli in (*a*), (*b*) and (*c*); (*f*) in groups of three, one member of which was of low intensity while the others were of the intensities of the stimuli in (*a*), (*b*) and (*c*). In the cases where the low intensities were used, the reagent was asked to react to the stimulus of low intensity. Finally for purposes of comparison reactions were had (*g*) to the simultaneous groups and pairs of stimuli with instructions in one course of the experiment to react to one of their members, and in the other to react to them as a whole.

After the foregoing description of the apparatus the presentation of the stimuli may not be difficult to understand. Fig. 1, with an exception noted later in regard to the location of the sound contact, shows the position of the large wheel and of the various contacts when the three simultaneous stimuli are given. The light is squarely behind the aperture through which the reagent observes. The second brush of the double contact brush (*s*) which closes the circuit to the sound hammer (*S*) has just passed upon the brass strip and causes the stroke of the hammer. Simultaneous with these events the adjustable slide (*ad.s.*) raises the arm of the electric circuit breaker (*E*) causing the reagent—whose right forefinger rests on the cathode in the reaction key (K_r) and whose left hand is immersed in the saline solution (*an*)—to receive a shock. The operator's signal comes about one and one half to two seconds before the occurrence of the stimuli. This interval is more than one revolution of the wheel. During this interval the operator, having started the chronoscope clock-work when he gave the signal, closes the three stimulus keys (K_l, K_e, K_s), and shortly afterward the chronometric key (K_{ch}). When the sound hammer strikes the base (*b*), that is, simultaneously with the sound and light stimuli, the current is

thrown into the chronoscope. The circuit is again broken when the reagent lifts his finger from the reaction key.

The interval of from one and a half to two seconds has been experimentally determined as the most favorable for obtaining regular reaction-times.[1] Martius[2] found that reactions with an irregular interval between signal and reaction stimulus result in a time intermediate between that with a regular signal and with no signal at all. Estel[3] has found that the most favorable interval is two and one fourth seconds; Wundt[4] places it at two and one half; Mehner[5] and Glass[6] agree with Wundt on the interval of two and one half seconds; Lange[7] thinks that two seconds is the best interval between signal and stimulus; Bertels[8] found that it took the reagent two and three eighths seconds to get properly prepared for the stimulus after the signal had been given; Dwelshauvers[9] varied the interval between signal and stimulus from a second and a half to three and to six seconds, and found for motor and sensory reactions the shortest time at a second and a half; Bliss[10] used an interval of two and one half seconds; della Valle[11] found very favorable intervals at two and six seconds, and a recent investigator, Breitwieser,[12] has found that the optimal interval between the signal and stimulus, and the one most often preferred by his subjects, was a period of from one to four seconds.

Bliss[13] states: ''Experiment has shown that when the interval

[1] Cattell, ''Aufmerksamkeit und Reaction,'' *Phil. Stud.*, **3**, 404, 1886. Also, ''The Time Taken up by Cerebral Operations,'' *Mind*, **11**, 239, 1886.

[2] ''Über den Einfluss der Intensität der Reize auf Reactionzeit der Klänge,'' *Phil. Stud.*, **7**, 469, 1891.

[3] ''Neue Versuche über den Zeitsinn,'' *Phil. Stud.*, **2**, 37, 1885.

[4] ''Physiol. Psych.,'' 3 ed., **2**, 361, 1887.

[5] ''Zur Lehre vom Zeitsinn,'' *Phil. Stud.*, **2**, 560, 1886.

[6] ''Kritisches und Experimentelles über den Zeitsinn,'' *Phil. Stud.*, **4**, 454, 1888.

[7] ''Beiträge zur Theorie der sinnlichen Aufmerksamkeit,'' *Phil. Stud.*, **3**, 492, 1888.

[8] ''Versuche über die Ablenkung der Aufmerksamkeit,'' Inaug. Diss., Dorpat, 1889.

[9] ''Untersuchungen zur Mechanik der Activen Aufmerksamkeit,'' *Phil. Stud.*, **6**, 225, 1891.

[10] ''Investigations in Reaction-time and Attention,'' *Yale Stud.*, **1–4**, 17, 1892–6.

[11] ''Der Einflusz der Erwartungszeit auf die Reactionsvorgänge,'' *Psych. Stud.*, **3**, 295, 1907.

[12] ''Attention and Movement in Reaction-time,'' ARCHIVES OF PSYCHOLOGY, **18**, 36, 1911.

[13] ''Investigations in Reaction-time and Attention,'' *Yale Stud.*, **1–4**, 16, 1892–6.

between warning and stimulus is always the same the mind is soon able to estimate the interval correctly and always reacts just at that time whether it hears the stimulus or not.'' Woodworth:[14] ''If the stimulus follows the signal at an irregular interval, the reaction-time is not so short as when the procedure is regular. If, indeed, the procedure is so regular that the moment of the stimulus can be exactly anticipated, the movement may be made to coincide in time with the stimulus, and the whole character of the experiment be thus changed. This result is avoided by varying the preliminary interval within narrow limits. The most favorable interval between the ready signal and the stimulus is one or two seconds; a shorter time does not allow the subject to prepare himself fully for the stimulus, while a longer period than two seconds allows more time than was needed and so affords a chance for wandering of the attention.'' In the present experiment the interval (never more than two seconds) was by necessity varied as it depended upon the experimenter's getting a view of the lamp as it came on the rim of the revolving wheel. This was a somewhat variable event and answered the general requirement for an interval, viz., that it vary, but within narrow limits.

§ 2. *Pairing and Grouping of Simultaneous Stimuli and Instructions to Reagents.*—The stimuli—light, electric shock and sound—were of what may be called approximate medium intensity. The intensity of the light was directly determined by photometric methods and was found to be about as certified by the makers of the bulb, 2 c.p. This raw intensity was reduced by the use of the ground glass and blue glass slides mentioned in the preceding division, and gave an available intensity of a little more than 1.5 c.p. This intensity was (within limits noted in the previous division) kept constant by the use of a photometer beside the aperture through which the reagent received the light stimulus.

The sound was determined by the distance of the electro-magnet hammer's fall which was readily measured on the mm. scale beside it. Of the stimuli used the light stimulus alone could not be varied in intensity mechanically, but did tend to vary, as has been pointed out, with the light in the experimental room, which variation was guarded against by the use of the photometer on the reagent's table. This constancy of the light stimulus made it necessary, in choosing the intensities for the sound and shock stimuli, to attempt to equate them with the light. Sounds might be readily equated by determined laws,[15] and lights by the use of the photometer, but there

[14] ''Elements of Physiological Psychology,'' 481, 1911.

[15] Starke, ''Zum Mass der Schallstärke,'' *Phil. Stud.*, **5**, 157, 1888–9.

is no sure way of equating disparate stimuli. Nevertheless, it was found that if the sound was very strong the light seemed in relation to it very faint; that if the intensity of the sound were then reduced the light seemed relatively to increase in intensity. Hence, by gradually reducing the sound it was possible to get an intensity after a while that did not *seem* more intense or prominent than the light. Such a sound was found also by working up from a very low intensity (in which case the light seemed relatively more intense than the sound) to one that seemed no more prominent than the light. Either of these intensities of sound was then called equal in intensity to the light. In a like manner the intensity of the shock was put into such relation to the intensity of the light that one was no more prominent than the other. Thus it can be said only loosely that the three disparate stimuli were of the same intensity.

The intensities thus chosen to agree with the light are called medium, since they are located between the noticeably low and the noticeably high intensities. It was possible to keep these intensities constant to a considerable degree. The constancy of the light was guarded by the use of a photometer. It was possible to reproduce its intensity by adjusting the curtains of the experimental room save on very dark days when the intensity was very noticeably greater. The sound was theoretically reproduced in every case by resetting the hammer according to a mark on the mm. scale beside it which mark corresponded to the initial position of the hammer for the production of the medium sound.

The sound as above determined was retained for all the subjects and was called the medium intensity that agreed with the light, and no objections were raised as to its parity with the light by any of the subjects. But the attempt to equate the electric shock intensity with the light and sound was a more difficult task. In the first place there were great individual differences in sensitiveness to this stimulus. Some were hypersensitive and could tolerate at first only a very weak shock. It was impossible to use this weak stimulus, as an adaptation to it soon made it almost disappear. It was possible, however, after several trials to get an intensity which seemed relatively constant for the experimental period. This fluctuation of the shock intensity was purely subjective or sensational, as there was nothing in the action of the apparatus or of the induction coil that would produce noticeable variations in the shock intensity. The point on the millimeter scale beside the adjustable coil to which its index pointed was marked for a given subject and retained as the position for giving this subject the medium electric shock stimulus. It was found necessary, however, at the beginning

of each series of experiments on different days, to test the subject for this point. Considerable variation was found in its location—it not unusually varied 1 cm. above or below the point decided upon during the previous experiment. It sometimes happened that after this point had thus been determined and in the middle of the experimental period that the reagent would ask the experimenter to re-locate the shock intensity as the light and sound intensities had become so much more prominent than usual. The correction that was made in such cases was in the direction of an increase in the shock intensity—the request was rarely for a diminshed shock intensity. As was said, this error in the judgment of the stimulus is to be ascribed not to changes in the induced or primary electric currents, but to organic or nervous changes in the subject, and consequently to his inability to get again very nearly the same sensation he had before. Cattell[16] well accounts for this inability: "Why should not the same stimulus be accompanied by the same sensation? The natural answer is that the conditions do not remain the same. In the first place the stimulus itself can not be kept exactly constant. Lights are always variable, and sounds and touches can not be exactly reproduced. Temperatures and smells are especially inconstant. Weights may remain nearly the same, but the manner of lifting them is always different. We have, therefore, a variable stimulus which in part accounts for the variation in sensation. In the second place the nervous mechanism is contantly changing. The sense organ is rhythmically exhausted and restored, and is subject to various irregular alterations. The nerves and paths of conduction in the brain would transmit more or less of the energy of the stimulus according to their ever-changing condition. Lastly, the brain centers immediately concerned with perception alter greatly in metabolism. These latter changes are best known to us on the side of consciousness; there is a more or less regular rhythm in attention, and very numerous irregularities due to fatigue, interest, etc. These sources of variation will sufficiently account for the fact that the same sensation does not recur."

§ 3. *Method and Results.*—After the intensities of the stimuli had been approximately equated they were presented in three different manners to the reagent. In one of these the stimuli were presented alone; in another, they were presented in pairs, while in the third manner they were in groups. In this paper the single stimuli are represented by the letters: *L, E, S,* meaning light, electric shock and sound, respectively. In the tables *L* means that the reaction was to light alone; *E,* to shock alone, and *S,* to sound alone. *LE*

[16] "On Errors of Observation," *Am. Jour. Psych.*, **5**, 283, 1892–3.

means that the reaction was to simultaneous light and electric shock, the subject having been instructed to react to the stimulus represented by the first letter, viz., light: *LS,* to simultaneous light and sound, the subject having been instructed to react to the light: *LSE,* reaction to simultaneous light, sound and shock, the subject having been instructed to single out (if possible) the light. *EL* signifies that the stimuli were simultaneous shock and light, the subject instructed to single out the shock, if possible, for reaction; *ES,* simultaneous shock and sound, with instructions to react to the shock if possible; *ELS,* simultaneous shock, light and sound, with instruction to react to the shock. Likewise *SL* represents simultaneous sound and light with instructions to react to the sound; *SE,* simultaneous sound and shock with reaction to sound; and *SLE,* simultaneous light, sound and shock, with reaction to sound.

In Table I., each of the entries in the first three pairs of columns represents the average of 100 reactions to the respective single,

TABLE I

Reaction to Single, Paired and Grouped Simultaneous Stimuli

Subjects	*A*		*P*		*T*			
Stimuli	Av.	M.V.	Av.	M.V.	Av.	M.V.	Avs.	
Light	177.5	*18.0*	164.6	*11.3*	181.1	*12.1*	174.4	*13.8*
LE	160.9	*19.5*	137.3	*12.8*	160.5	*10.1*	152.9	*14.1*
LS	149.0	*14.5*	126.6	*11.9*	156.5	*9.2*	144.0	*11.8*
LES	128.7	*14.3*	117.0	*8.7*	149.1	*8.2*	131.6	*10.4*
Electric	169.9	*21.8*	163.4	*18.1*	177.9	*10.9*	170.4	*16.6*
EL	156.9	*17.7*	156.8	*14.9*	170.6	*11.5*	161.4	*14.7*
ES	148.8	*19.7*	155.3	*16.6*	152.9	*10.1*	152.3	*15.4*
ELS	134.7	*21.1*	134.9	*21.1*	148.9	*8.6*	139.5	*16.9*
Sound	156.8	*15.8*	132.9	*9.1*	170.3	*10.2*	153.3	*11.7*
SL	148.0	*18.5*	126.2	*15.5*	171.6	*10.7*	148.6	*14.9*
SE	147.4	*18.0*	129.2	*11.5*	149.6	*8.7*	142.0	*12.7*
SLE	131.8	*14.7*	120.9	*7.9*	149.1	*13.1*	133.9	*11.9*

paired and grouped stimuli, for each subject, *A, P, T.* The last pair of columns gives the averages of the three subjects' reactions to the stimuli, and consequently are derived from 300 single reactions.

The M.V.'s are the average deviations of the 100 single or individual reactions from the average.

Fig. 2 is a graphic representation of Table I. It shows that the reaction-times to the single stimuli are longer on the whole than the reaction-times to the paired and grouped stimuli, and that the reaction-times to the paired stimuli are longer than those to the grouped stimuli.

A summary of Table I. is obtained by running down the column

of gross averages, headed "Avs." in the table. There is shown a gradual decrease from *L* to *LE* to *LS* and *LSE*; from *E* to *EL* to *ES* to *ELS*; from *S* to *SL* to *SE* to *SLE*, with one exception. The reaction-times to the pairs and groups of stimuli are shorter regardless of which member of the pairs or groups was the designated element to which the reaction was to be made. Inspection will show

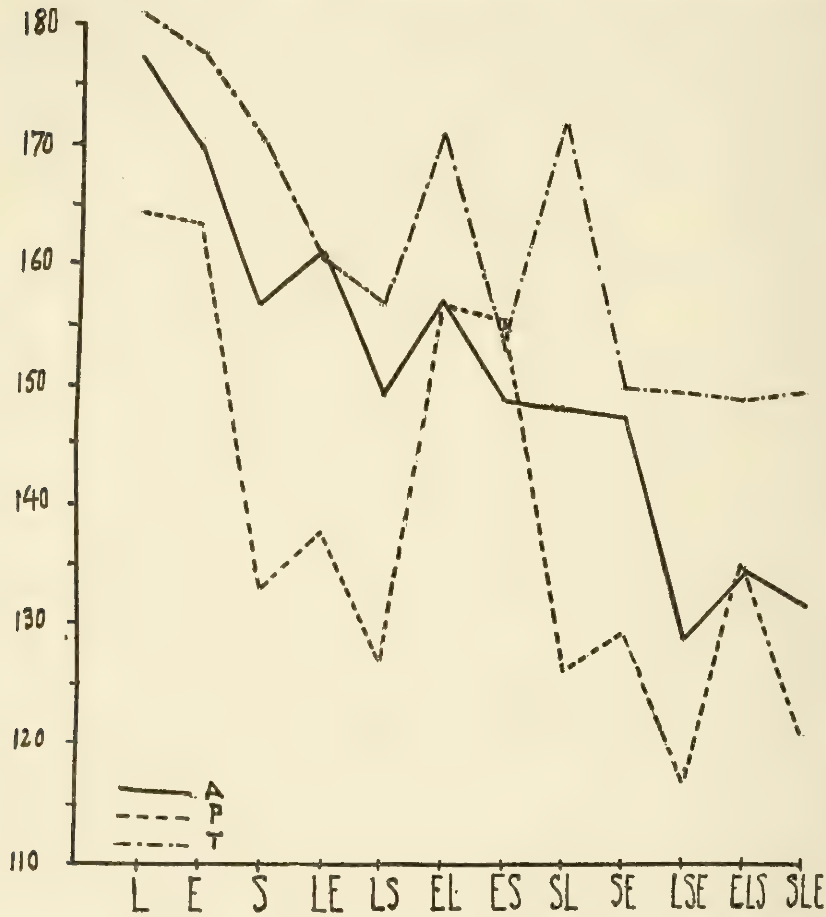

FIG. 2. Graphic Representation of Table I.

that the different components of the pairs and groups were at different times the designated stimulus for the reaction, the first letter of the pair or group representing in each case in the table the element to which the attention of the reagent had been called. That is to say, the reagent was "set" for that stimulus accompanied simultaneously by the other stimuli. Whether or not he was able

to react to this designated stimulus and to leave the other stimuli outside of the direct attention process is the subject of a later division of this article.

TABLE II

REACTION TO THE SINGLE STIMULI

	Light Stimulus Av.	M.V.	Electric Stimulus Av.	M.V.	Sound Stimulus Av.	M.V.
A	175.5	*18.0*	169.9	*21.8*	156.8	*15.8*
P	164.6	*11.3*	163.4	*18.1*	132.9	*9.1*
T	181.1	*12.1*	177.9	*10.9*	170.3	*10.2*
	173.7	*13.8*	**170.4**	*16.6*	**153.3**	*11.7*

As shown by Table II., the reaction-time to light is the longest for the three reagents without exception; the reaction-time to sound is the shortest, and to the electric shock, medium. The table seems also to indicate that the reaction-times for shock and light are closer together than those for shock and sound. These general relations are shown also in the Gross Averages. These relative reaction-times are important as they will figure in the subsequent tables of this division, and lend a great deal toward the interpretation of them. These same facts have been shown by the majority of experimenters who have investigated the reaction-times to light, electric shock and sound, as is shown by the following table from Wundt:[17]

	Sound	Light	Shock
Hirsch	149	200	182
Donders	180	188	154
Hankel	150	224	154
Wundt	167	222	201
Exner	136	150	133
v. Kries	120	193	117
Auerbach	122	191	146
Cattell	125	150	...

It is seen from this table of Wundt's that Exner, v. Kries, and Donders found the electric shock stimulus to produce the shortest reaction-time. For the other experimenters the sound stimulus produced the shortest reaction-time, and electric shock, save for Wundt, the next shortest reaction-time, and light for all produced the longest reaction-time.

In Table III., opposite *L, E, S,* are given the reaction-times to the single stimuli for the subjects *A, P, T,* respectively. In the decrease column is given the reduction of time in sigmas brought about by the reactions to the simultaneous paired stimuli as com-

[17] *Physiol. Psych.*, **2**, 312, 1893. In connection with the table, the sources from which it was derived are quoted.

TABLE III

Comparison of Reaction-times to Paired Stimuli with Reaction-times to Single Stimuli

Subject:	A Av.	A M.V.	A Decrease from				P Av.	P M.V.	P Decrease from				T Av.	T M.V.	T Decrease from			
L	177.5	*18.0*					164.6	*11.3*					181.1	*12.1*				
E	169.9	*21.8*					163.4	*18.1*					177.9	*10.9*				
S	156.8	*15.8*					132.9	*9.1*					170.3	*10.2*				
LE	160.9	*19.5*	*L*	16.6	*E*	9.0	137.3	*12.8*	*L*	27.3	*E*	26.1	160.5	*10.1*	*L*	20.6	*E*	17.4
		P.E.		*2.2*		*2.4*				*1.8*		*1.9*				*1.3*		*1.2*
LS	149.0	*14.5*		28.5	*S*	7.8	126.6	*11.9*		38.0	*S*	6.3	156.5	*9.2*		24.6	*S*	13.8
		P.E.		*1.9*		*1.8*				*1.4*		*1.2*				*1.3*		*1.2*
EL	156.9	*17.7*	*E*	13.0	*L*	20.6	156.8	*14.9*	*E*	6.6	*L*	7.8	170.6	*11.5*	*E*	7.3	*L*	10.5
		P.E.		*2.3*		*2.1*				*2.0*		*1.6*				*1.3*		*1.4*
ES	148.8	*19.7*		21.1	*S*	8.0	155.3	*16.6*		8.1	*S*	—22.4	152.9	*10.1*		25.0	*S*	17.4
		P.E.		*2.4*		*2.1*				*2.0*		*1.6*				*1.2*		*1.2*
SL	148.0	*18.5*	*S*	8.8	*L*	29.5	126.2	*15.5*	*S*	6.7	*L*	38.4	171.6	*10.7*	*S*	—1.3	*L*	10.5
		P.E.		*2.0*		*2.2*				*1.5*		*1.6*				*1.2*		*1.4*
SE	147.4	*18.0*		9.4	*E*	22.5	129.2	*11.5*		3.7	*E*	34.2	149.1	*13.1*		20.7	*E*	28.5
		P.E.		*2.0*		*2.4*				*1.2*		*1.8*				*1.4*		*1.4*

pared with the reactions to the separate stimuli which compose the pairs. The P.E.'s of the differences[18] are given in connection with them. With three exceptions the reaction-times to the paired simultaneous stimuli are shorter than to any of their members reacted to alone. These exceptions occur for the subjects *P* and *T* and are designated by the negative sign before the amount of decrease.

These results do not agree with the statement of Dunlap and Wells,[19] namely, that they have found the reaction-times to simultaneous light and sound a little longer than to sound alone.

Table IV. gives the average of the decreases in the reaction-times of the three reagents *A, P, T,* to the simultaneous paired stimuli from the reaction-times to their members alone. This table shows in a compact form what is shown by the larger table from which it is derived (Table III.): That the stimulus *S,* which produces the shortest reaction-time causes a greater decrease in the reaction-time to *L* than does *E* which produces a slower reaction. It shows also that *S* brings about a greater reduction in the reaction-time to *E* than is produced by *L.* It shows also that in the groups *SL* and *SE, E,* which produces a reaction whose time is medium between those to light and to sound, causes a greater reduction in time when added to *S* than is produced by *L* which causes the slowest reaction-time.

In general, then, Tables III. and IV. show that the reaction-time

TABLE IV

AVERAGE DECREASE OF REACTION-TIMES TO PAIRS FROM REACTION-TIMES TO THEIR INDIVIDUAL MEMBERS

LE	from	*L*	21.5	*EL*	from	*E*	8.9	*SL*	from	*S*	4.7
		P.E.	*.96*				*1.1*				*.92*
		E	17.5			*L*	12.9			*L*	26.1
		P.E.	*1.0*				*.97*				*.98*
LS		*L*	30.3	*ES*		*E*	18.1	*SE*		*S*	11.2
		P.E.	*.88*				*1.1*				*.84*
		S	9.3			*S*	1.0			*E*	5.6
		P.E.	*.81*				*.94*				*1.01*

Each decrease is from the averages of 300 reaction-times.

[18] Thorndike, "Mental and Social Measurements," 144, 1904: "The unreliability of a difference between two measures equals in fact the square root of the sum of the squares of the unreliabilities of the measures themselves." That is, the P.E. of the difference between two measures whose M.V.'s are given

$$=\sqrt{\left(\frac{.84435\ \text{M.V.}_A}{\sqrt{n}}\right)^2+\left(\frac{.84435\ \text{M.V.}_B}{\sqrt{n}}\right)^2}.$$

[19] "Reactions to Visual and Auditory Stimuli," *Psych. Rev.*, 17, 319, 1910.

to a pair of simultaneous stimuli is shorter than the reaction-time to either member of the pair alone, and that the addition of another stimulus to a given stimulus, reduces the reaction-time, and reduces it in accordance with the reaction-time to the stimulus added—the addition of light (which causes the slowest reaction-time) to sound produces a less decrease than the addition of the electric shock (which produces a faster time than light when reacted to alone); and the addition of sound produces the greatest reduction in reaction-time.

Tables V. and VI. show these decreases in per cents.

TABLE V

PER CENTS OF DECREASE OF REACTION-TIMES TO GROUPS FROM REACTION-TIMES TO THEIR INDIVIDUAL MEMBERS

Subjects			Per Cent. of Decrease *A*	*P*	*T*		Per Cent. of Decrease *A*	*P*	*T*
LE	from	*L*	9.3	16.5	11.3	*E*	5.2	15.9	9.2
LS			16.0	23.7	13.5	*S*	4.9	4.8	8.1
EL		*E*	7.6	4.0	4.1	*L*	11.6	4.7	5.7
ES			12.2	4.9	14.0	*S*	5.1	—16.7	10.2
SL		*S*	5.6	5.0	—.7	*L*	16.6	20.9	5.7
SE			5.9	2.8	12.1	*E*	13.2	—20.1	10.8

TABLE VI

AVERAGE PER CENT. OF DECREASE OF REACTION-TIMES TO GROUPS FROM REACTION-TIMES TO THEIR INDIVIDUAL MEMBERS

LE	from	*L*	12.3	*EL*	from	*E*	5.2	*SL*	from	*S*	3.3
		E	10.1			*L*	7.2			*L*	14.4
LS		*L*	17.7	*ES*		*E*	10.3	*SE*		*S*	6.9
		S	6.2			*S*	—.46			*E*	3.3

In Table VII., under *L* is given the amount of decrease of reaction-time for reagent *A* to *LSE* from that of his reaction to *L* alone. Likewise, under *S* and *E* are given the similarly obtained decreases. Under *LS* is given the amount of decrease of the reaction-time for reagent *A* to *LSE* from his reaction to *LS*. Also under *LE* and *SE* are the similarly obtained decreases. The average of the decreases for the three subjects are also given. In Table VIII. the decreases are converted into per cents.

These tables and curves show that the reaction-times of the three reagents to the groups of three simultaneous stimuli are shorter than the reaction-times to the single stimuli (Table II.), and also that they are shorter than the reaction-times to the pairs of simultaneous stimuli (Table III.). Tables VII. and VIII. show also that the amount of the decrease of reaction-times to groups of three simultaneous stimuli is greater when computed from the reaction-

times to the single stimuli than when computed from the reaction-times to the groups of simultaneous stimuli. In the light of this, any member of the three simultaneous stimuli may be considered as having the added influence of the other two members, and, as will be shown later in the case of reactions to the pairs of simultaneous stimuli (Table IX.), this influence is according to the quickness or

TABLE VII

AMOUNTS OF DECREASE OF REACTION-TIMES TO GROUPED STIMULI FROM THE REACTION-TIMES TO THEIR INDIVIDUAL MEMBERS AND PAIRS

Stimuli			Amounts of Decrease from								
	LSE		*L*	*S*	*E*	*LS*	*LE*	*SE*	*SL*	*EL*	*ES*
	Av.	M.V.									
A	128.7	*14.3*	48.8	28.1	41.2	20.3	32.2	18.7	19.3	28.2	20.1
		P.E.	*1.9*	*1.8*	*2.2*	*1.7*	*1.9*	*1.9*	*2.0*	*1.9*	*2.0*
P	117.0	*8.7*	47.6	15.9	46.4	9.6	20.3	12.2	9.2	39.8	38.3
		P.E.	*1.2*	*1.0*	*1.7*	*1.2*	*1.3*	*1.2*	*1.5*	*1.4*	*1.6*
T	*149.1*	*8.2*	32.0	21.2	28.8	7.4	11.4	0.5	22.5	21.5	3.8
		P.E.	*1.2*	*1.1*	*1.1*	*1.0*	*1.1*	*1.3*	*1.1*	*1.2*	*1.1*
Av.	**131.6**	*10.4*	**42.8**	**21.7**	**38.8**	**12.4**	**21.3**	**10.4**	**17.0**	**29.8**	**20.7**
		P.E.	*.84*	*.76*	*.95*	*.76*	*.86*	*.79*	*.88*	*.87*	*.90*
	ELS		*E*	*L*	*S*	*EL*	*SE*	*LS*	*LE*	*SE*	*SL*
A	134.7	*21.1*	35.2	42.8	22.1	22.2	14.1	14.3	26.2	12.7	13.3
		P.E.	*2.5*	*2.3*	*2.2*	*2.3*	*2.4*	*2.1*	*2.3*	*2.3*	*2.3*
P	134.9	*21.1*	28.5	29.7	−2.0	21.9	20.4	−8.3	2.4	21.9	−8.7
		P.E.	*2.3*	*2.0*	*1.9*	*2.2*	*2.3*	*2.0*	*2.1*	*2.0*	*2.4*
T	148.9	*8.6*	29.0	51.2	29.0	21.7	11.6	0.7	22.7	21.7	4.0
		P.E.	*1.1*	*1.2*	*1.1*	*1.2*	*1.1*	*1.1*	*1.1*	*1.3*	*1.1*
Av.	**139.5**	*16.9*	**30.9**	**41.2**	**16.3**	**21.9**	**15.3**	**2.2**	**17.1**	**18.7**	**2.8**
		P.E.	*1.1*	*1.0*	*1.0*	*1.1*	*1.1*	*1.0*	*1.1*	*1.0*	*1.1*
	SLE		*S*	*L*	*E*	*SL*	*ES*	*LE*	*LS*	*ES*	*EL*
A	131.8	*14.7*	25.0	65.7	38.1	16.2	15.6	29.1	17.1	17.0	25.1
		P.E.	*1.8*	*1.9*	*2.2*	*1.9*	*1.9*	*2.0*	*1.7*	*2.0*	*1.9*
P	120.9	*7.9*	12.0	43.7	42.5	5.3	8.3	16.4	5.7	34.4	35.9
		P.E.	*1.0*	*1.1*	*1.6*	*1.5*	*1.2*	*1.3*	*1.2*	*1.5*	*1.4*
T	149.1	*13.1*	21.2	32.0	28.8	22.5	0.5	11.4	7.4	3.8	21.5
		P.E.	*1.4*	*1.5*	*1.4*	*1.4*	*1.5*	*1.4*	*1.3*	*1.4*	*1.5*
Av.	**133.9**	*11.9*	**19.4**	**47.1**	**36.4**	**14.6**	**8.1**	**18.9**	**10.0**	**18.4**	**27.5**
		P.E.	*.81*	*.88*	*.99*	*.93*	*.85*	*.90*	*.82*	*.95*	*.92*

slowness of the reaction-times to the other two as a pair. For example, the reaction-time to *LSE* may be considered as the reaction-time to *L* facilitated by an accompanying element, the reaction-time to the pair *SE*. It may be looked upon also as the reaction-time to *SE* facilitated by the reaction-time to *L*. As it shown in Tables VII. and VIII., *L*, whose independent reaction-time is longer than that of *SE*, will nevertheless when added to *SE* decrease its reaction-time. The general effect seems to be the same as increasing the

TABLE VIII

PER CENTS OF DECREASE OF REACTION-TIMES TO GROUPED STIMULI FROM THE REACTION-TIMES TO THEIR INDIVIDUAL MEMBERS

LSE	*L*	*S*	*E*	*LS*	*LE*	*SE*	*SL*	*EL*	*ES*
A	21.8	17.9	24.2	13.6	20.0	12.7	13.0	11.6	13.7
P	28.9	11.9	28.3	7.5	14.7	9.4	7.2	25.5	24.6
T	17.6	12.4	16.1	4.7	7.1	3.3	13.1	12.6	2.4
Av.	**22.7**	**14.0**	**22.8**	**8.2**	**13.9**	**8.4**	**11.1**	**16.5**	**13.5**
ELS	*E*	*L*	*S*	*EL*	*ES*	*LS*	*LE*	*SE*	*SL*
A	20.7	24.1	12.8	14.1	9.4	9.5	16.2	8.6	9.0
P	17.5	18.0	−1.5	13.9	13.1	−6.6	1.7	16.9	−6.8
T	16.2	28.2	17.0	12.7	7.5	4.4	14.1	14.5	2.3
Av.	**18.1**	**23.4**	**9.4**	**13.5**	**10.0**	**2.4**	**10.6**	**13.3**	**1.5**
SLE	*S*	*L*	*E*	*SL*	*SE*	*LE*	*LS*	*ES*	*EL*
A	15.9	37.0	22.4	10.9	10.5	18.0	11.4	11.3	15.3
P	9.0	26.5	26.0	4.2	6.4	11.9	4.5	22.1	22.9
T	12.4	17.7	16.1	13.1	3.3	7.1	47.2	2.4	12.6
Av.	**12.4**	**27.0**	**21.5**	**12.7**	**6.7**	**12.3**	**21.0**	**11.9**	**16.9**

intensity, extensity or area of the stimulus. In character, however, it differs from the usual increase of intensity or extensity of a stimulus, which excites but one sense organ, by exciting three disparate sense-organs.

Piper,[20] Cattell and Dolley,[21] and Froeberg[22] have shown that an increase in the area of a visual stimulus causes a decrease in the reaction-time, which is the same relation that exists between an increase of the intensity of a stimulus and the reaction-time. A comparison of the reaction-times shows the least decrease of reaction-time when the reaction-time to the three simultaneous stimuli is subtracted from that to the various simultaneous pairs. This is also what might be expected in the light of our obtained results, since we may consider that here there has been added one stimulus instead of two, making the situation again analogous to an increase of intensity or extensity of stimulus, but not so much as before. Tables VII. and VIII. show under captions of average for the three reagents, the absolute amounts of decrease and the per cents of decrease produced by *LSE, ELS* and *SLE* over the single and paired simultaneous stimuli. Reference to this table makes it clear that the decrease in the reaction-times to the three simultaneous

[20] "Ueber die Abhängigkeit des Reizwertes leuchtender Objekte von ihrer Flächen bezw. Winkelgrösse," *Ztsch. f. Psychol.*, **32**, 98, 1903.

[21] "On Reaction-times and the Velocity of the Nervous Impulse," *Nat. Acad. of Sci.*, **7**, 409, 1893.

[22] "The Relation between the Magnitude of the Stimulus and the Time of Reaction," ARCHIVES OF PSYCHOLOGY, **8**, 1907.

stimuli is greater when calculated from the reaction-times to the single stimuli, than when it is calculated from the reaction-times to the simultaneous paired stimuli.

The small table below gives these results in a more compact form. The table represents the averages of the general averages of the amount of decrease for the three reagents, and each decrease is therefore derived from 900 individual reaction-times.

	Decrease from Reaction-time to Single Stimuli	Decrease from Reaction-time to Pairs
Reaction-time to *LSE*	34.4σ	14.7σ
Reaction-time to *ELS*	29.7	12.9
Reaction-time to *SLE*	30.9	13.8

It is important to point out that the average decreases of *LSE, ELS* and *SLE* over the single and paired stimuli are fairly close together, and seem to indicate that the reagents did not single out the designated members of the groups and pairs of simultaneous stimuli as the chief object of their reactions. Many times while attempting to react to the designated stimuli, the reagents became confused and stated that they were very uncertain about their reactions and that they could do better if they gave the matter less attention. The confusion and uncertainty was shown objectively by premature and tardy reactions. That the reaction is made most smoothly when it is the least interfered with by consciousness indicates its reflex nature. This is the view of Cattell and will be discussed in another division of this paper.

It was shown in Table II. that the relative reaction-times for the three stimuli, light, electric shock and sound, were: light, slowest; electric shock, medium; and sound, fastest.

Table IX. shows the facilitation that occurs when another stimulus is presented simultaneously with a given one. The table treats each of the three stimuli as it acts with each of the other two of the group. In this way the effect of each stimulus as an accelerator is shown.

Explanation of the table: *S* which produces for the three reagents the fastest reaction-time, when added to (presented simultaneously with) *L,* which produces the slowest reaction-time, reduces *L*'s time for subject *A* 28.5$^\sigma$ or 19.1 per cent. Likewise, *E,* which produces the medium reaction-time when reacted to alone, when added to *L,* decreases *L*'s time 16.6$^\sigma$ or 10.3 per cent. Thus it is seen that *S* which produces the fastest reaction-time reduces the reaction-time to *L* more when added to it than does *E,* whose effect is to produce a longer reaction-time than *S*. The pair of columns

called d_1–d_2 and per cent.-difference show the reduction in σ and per cent. produced by adding to a given stimulus, respectively, one of the other two stimuli.

TABLE IX

THE RELATIVE FACILITATION EFFECTS OF LIGHT, ELECTRIC SHOCK AND SOUND

	Fastest with Slowest				Medium with Slowest			d_1—d_2 and Per Cent.-difference	
	L	*LS*	d_1	Per Cent.	*LE*	d_2	Per Cent.		
A	177.5	149.0	28.5	19.1	160.9	16.6	10.3	11.9	8.8
		P.E.	*1.9*			*2.2*			
P	164.6	126.6	38.0	30.8	137.3	27.3	20.6	10.7	10.2
		P.E.	*1.4*			*1.8*			
T	181.1	156.5	24.6	15.7	160.5	20.6	11.3	4.0	4.4
		P.E.	*1.3*			*1.3*			
Av.	**173.7**	**144.0**	**30.3**	**21.8**	**152.9**	**21.5**	**13.8**	**8.8**	**7.8**
		P.E.	*.88*			*.96*			

	Fastest with Medium				Slowest with Medium			d_1—d_2 and Per Cent.-difference	
	E	*ES*	d_1	Per Cent.	*EL*	d_2	Per Cent.		
A	169.9	155.3	21.1	12.4	156.9	13.0	7.7	8.1	4.7
		P.E.	*2.4*			*2.3*			
P	163.4	155.3	8.1	4.9	156.8	6.6	4.3	1.5	.6
		P.E.	*2.0*			*2.0*			
T	177.9	152.9	25.0	15.0	170.6	7.3	5.2	17.7	9.8
		P.E.	*1.2*			*1.3*			
Av.	**170.4**	**157.3**	**18.1**	**10.7**	**161.4**	**8.9**	**5.7**	**9.1**	**5.0**
		P.E.	*1.1*			*1.1*			

	Medium with Fastest				Slowest with Fastest			d_1—d_2 and Per Cent.-difference	
	S	*SE*	d_1	Per Cent.	*SL*	d_2	Per Cent.		
A	156.8	147.4	9.4	5.9	148.0	8.8	5.6	.6	.3
		P.E.	*2.0*			*2.0*			
P	132.9	129.2	3.7	2.7	126.2	6.7	5.0	—3.0	—2.3
		P.E.	*1.2*			*1.5*			
T	170.3	149.6	20.7	18.0	171.6	—1.3	—.7	32.0	17.3
		P.E.	*1.4*			*1.2*			
Av.	**153.3**	**142.0**	**11.2**	**8.8**	**148.6**	**4.7**	**3.7**	**9.8**	**5.1**
		P.E.	*.84*			*.92*			

In the case of adding *S* and *L,* respectively, to *E* (Fastest with Medium, and Slowest with Medium), it is found that *S* which produces the quickest reaction-time when reacted to alone reduces the time more when presented simultaneously with *E* than *L* does when presented simultaneously with it.

Adding *E* and *L,* respectively, to *S* (Medium with Fastest, and Slowest with Fastest): For subject *A* the reduction is insignificant, and for subject *P,* the reduction is negative, but for subject *T* the reduction is rather large. The average reduction is 9.8$^\sigma$ or 5.1 per cent. Two later tables (Table XXI. and Table XXIII.) show fair positive average reductions for the subjects *M, P* and *T, M* being a new subject.

The general contribution of this table seems to be that the amount of facilitating influence produced by the addition of a given stimulus to another is dependent upon the quickness or slowness of the reaction-time to the added stimulus when presented alone. That is to say, the addition of light to another stimulus causes the least decrease in reaction-time; the addition of electric shock produces a greater decrease than light, and the addition of sound produces the greatest decrease.

TABLE X

REACTION TO FIRST MEMBER OF PAIRED AND GROUPED STIMULI

	LE	*EL*	*d*	*LS*	*SL*	*d*	*SE*	*ES*	*d*
A	160.9	156.9	4.0	149.0	148.0	1.0	147.4	148.8	1.4
		P.E.	*2.1*			*1.9*			*2.2*
P	137.3	156.8	19.5	126.6	126.2	.4	155.3	129.2	26.1
		P.E.	*1.6*			*1.6*			*1.7*
T	160.5	170.6	10.1	156.5	171.6	15.1	152.9	149.6	3.3
		P.E.	*1.3*			*1.2*			*1.4*
Av.	**152.9**	**161.4**	**11.2**	**144.0**	**148.6**	**5.5**	**151.8**	**142.5**	**10.2**
		P.E.	*.99*			*.93*			*.97*

	LSE	*ELS*	*SLE*	Av.	Var.
A	128.7	134.7	131.8	131.7	*2.0*
P	117.0	134.9	120.9	124.2	*6.4*
T	149.1	148.9	149.1	149.0	*.7*
Av.	**131.6**	**139.5**	**133.9**	**134.9**	**3.0**

Table X. brings into direct comparison the reaction-times to groups of stimuli which consist of the same elements, and whose only difference consists in the fact that in one of the groups one of the common members was singled out for reaction, and in the the other the second of the common members was singled out as the object of the reaction-attention, during the interval of preparation for the stimulus. That is, *LE* means that the stimuli light and electric shock were simultaneously presented to the reagent who was instructed to react to the light, and under *LE* in Table X. is entered the average of one hundred such reaction-times. Under *EL* is entered the average of one hundred reaction-times to the same group of simultaneous stimuli obtained in a similar manner save that the reagent was requested to react to the electric shock. Under *d* is given the absolute differences between the reaction-times to the same simultaneous groups when the instructions were in one case to react to one member of the group and in the other to the second member of the group.

In the case of reaction to the simultaneous presentation of light, sound and electric shock, there are three permutations, furnishing in turn three respective reaction objects. These are shown in the

table under *LSE*, *ELS* and *SLE*. No differences are given but their average and the mean variations are set down.

Fig. 3 represents this table graphically, and indicates that the results so far obtained do not show that it is possible for a reagent to single out a given element of a pair or group of simultaneous

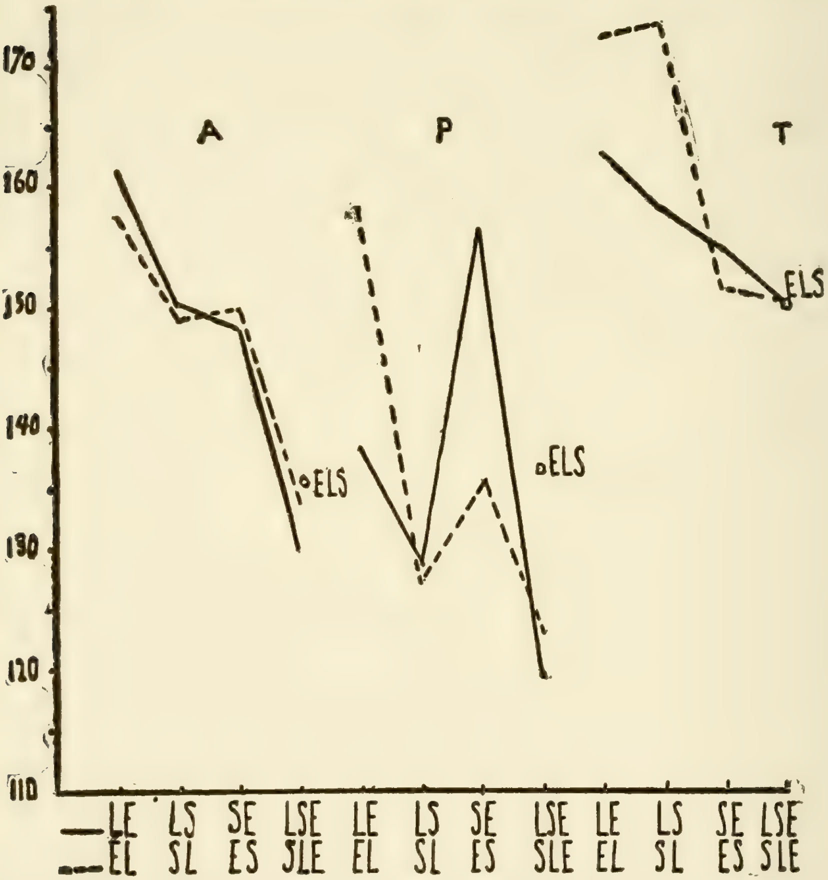

FIG. 3. Graphic Representation of Table X.

stimuli and react to it. When the reagent is properly "set" for the reaction, it makes no apparent difference which component of a multiple group he is told to notice. In the light of the previous results pertaining to the relative facilitation effects of the different stimuli used, and in the light of the long-known relative reaction-times to the stimuli employed in this experiment, it would naturally follow, if the reagent were able to single out a member of a pair or group as the object of his reaction, that there would result a very distinct difference between the reaction-times.

In Fig. 3, in the case of subject *P,* the great difference of the reaction-times to the simultaneous pairs *SE* and *ES* is due to an unusual sensitivity to the electric shock on the part of this subject. His introspection at this point was that the shock seemed to dominate. If subject *P* had been able to single out the designated members of the pairs in his reactions, the solid curve would have fallen below the broken one at this point. This question concerning the possibility of singling out a member of a group or pair of stimuli will receive attention later in this paper, and will be the subject of a special experiment.

IV.

Reaction to Simultaneous Stimuli after Correction for the Sound-distance

As was stated in the description of the apparatus, an electromagnet hammer was used to produce the sound stimulus. It was stated also that the hammer was placed near the experimenter for the purpose of convenience since during the course of an experiment it was necessary to open and close the hammer a number of times. This caused the hammer to be placed about four feet distant from the reagent. It will be recalled that the sound hammer closed the chronoscope circuit and threw the current of the gravity battery into the electromagnets of the chronoscope. It will be recalled also that the light was visible and the electric shock was produced simultaneously with the closing of the sound hammer circuit. In so far, then, as we are concerned just now, the sound was produced, the light was exposed and the shock given all in the same instant—physically they were all simultaneous events. But no account has been taken of the distance of the sound hammer from the reagent. This distance, about four feet, causes a tardiness of about 4^{σ} in the sound's getting to the reagent's ear. So far as this examination of the apparatus is at present concerned, two of the so-called simultaneous stimuli impinged upon the sense-organs at the same instant, namely, light and electric shock; the other, sound, reached the ear 4^{σ} afterwards.

Since further data were desirable touching the questions discussed in the preceding pages, it was decided to correct for the sound hammer distance and operate the experiment on subject *P*. The correction was made by advancing the sound hammer contact (*s*, Fig. 1) through a distance derived by dividing the sigma-units for one revolution of the wheel (W) by the circumference-units of the circular contact strip (W_1). It was found necessary to advance the sound hammer contact about three fourths of one centimeter. After this correction had been made the three stimuli were with regard to impingement on the sense organs, simultaneous stimuli.

As the tardiness of 4^{σ} occasioned by the time required for the sound to travel to the ear was much less than the mean variation of the reaction-times for subject *P*, its effect does not appear in the results which are recorded in Table XI.

With the exception that the reagent still seems hypersensitive to the electric shock, his reaction-times hold their previous relations to one another, viz., light, longest; sound, shortest; and electric shock medium. The graph of the results in Fig. 4 shows a decided decrease in all the reaction-times in which the electric shock is an element. For example, reaction to the pair *LE* which has usually

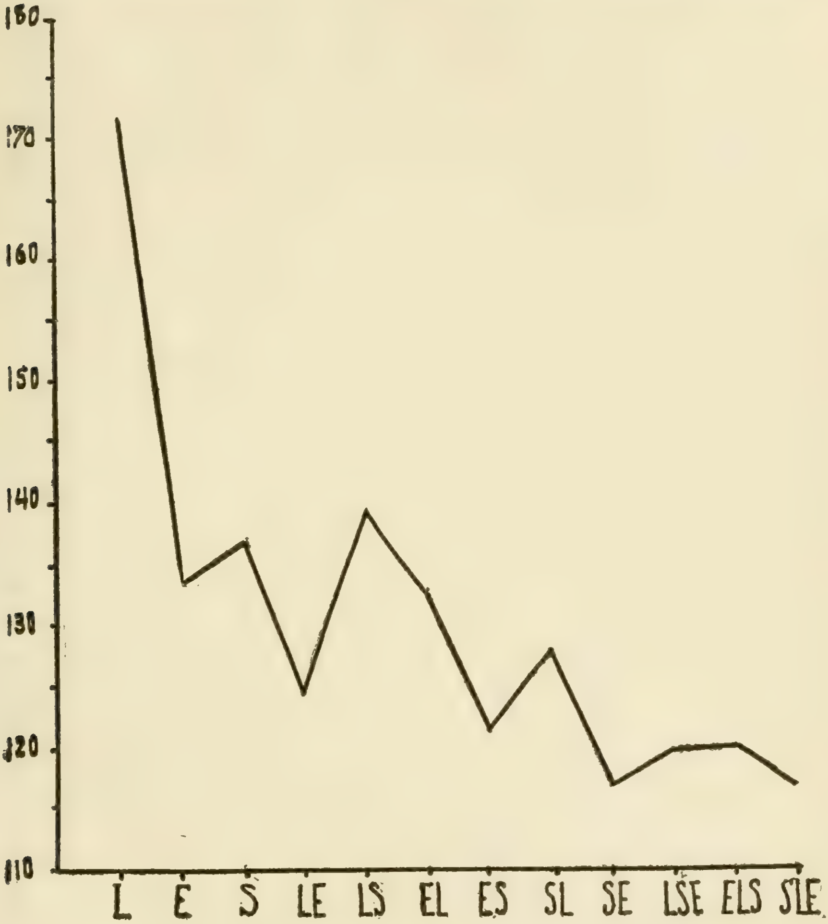

Fig. 4. Graphic Representation of Table XI.

given a longer reaction-time than reaction to *LS*, in this case gives a shorter time, and one almost as short as that to the combination *ES*.

The results in Tables XI.–XV. are drawn from 100 reaction-times to each of the stimuli-elements of the tables. The mean variations

are not drawn from the deviations of each of the 100 individual readings from the average as was the case in the preceding division. They are the averages of the deviations of each set of 10 single reaction-times from the average of the 100 individual reaction-

TABLE XI

Subject *P*. Reaction to Single, Paired and Grouped Stimuli After Correction for Sound Distance

Light		*LS*		*LE*		*LSE*	
171.7	*4.8*	139.4	*7.3*	124.4	*3.9*	119.6	*2.8*
Electric		*EL*		*ES*		*ELS*	
133.3	*7.0*	132.8	*11.0*	121.3	*3.0*	120.0	*4.2*
Sound		*SE*		*SL*		*SLE*	
136.7	*13.4*	116.9	*6.0*	132.5	*4.4*	116.7	*4.4*

times. The mean variations thus obtained are longer than the mean variations derived by averaging the deviations of all the individual variations from the average of each 10 reactions.

Table XII. shows that in the case of the paired simultaneous stimuli the reaction-times to the pairs are shorter without exception than the reaction-times to each stimulus of which the several pairs are composed. Because, as was stated, subject *P* had become un-

TABLE XII

Subject *P*. Comparison of Reaction-times to Paired Stimuli with Reaction-times to Single Stimuli

L	171.7	*4.8*				
E	133.3	*7.0*				
S	136.7	*13.4*				
			Decrease from			
			L	%	*S*	%
LS	134.9	*7.3*	36.8	21.3	1.8	13.0
		P.E.	*2.1*		*3.7*	
SL	132.5	*8.2*	39.2	22.8	4.2	3.6
		P.E.	*2.5*		*3.7*	
			L		*E*	
LE	124.4	*3.9*	47.3	27.5	8.9	6.6
		P.E.	*1.6*		*2.1*	
EL	132.8	*11.0*	38.9	22.6	.5	.4
		P.E.	*3.1*		*3.4*	
			S		*E*	
ES	121.3	*3.0*	15.4	11.2	12.0	9.0
		P.E.	*3.6*		*2.0*	
SE	116.9	*6.0*	19.8	14.4	16.4	12.3
		P.E.	*2.4*		*2.4*	

usually sensitive to the electric shock, the reductions in reaction-times shown by a pair of simultaneous stimuli are greatest in the case of pairs which contain the electric shock stimulus. The earlier experiments showed that for reagents *A, P* and *T* the reductions were greatest in the case of the pairs of simultaneous stimuli which contained the sound stimulus.

Table XIII. shows that the reaction-times to the simultaneous stimuli *LSE, ELS* and *SLE* are very much shorter than the reaction-times to their component members when presented alone. In this there is agreement with the tables of the previous division. The table shows also that the absolute decrease from the individual reaction-times to light is the greatest. Contrary to the finding of the previous division, the decrease from the individual reaction-time to sound is not as great as that from the individual reaction-time to electric shock. The table shows also that there is a positive decrease in the reaction-times to the groups of three simultaneous stimuli from those to pairs of simultaneous stimuli, save in the case of the reaction-times to sound and shock. As the decreases of reaction-times to the groups of three simultaneous stimuli from those to their single members, light, sound and electric shock, are quantitatively near each other, the earlier opinion as to the reagents' inability to single out one of the members of a group for reaction is further supported. Regardless of the fact that there is less reduction in the reaction-times to the groups from the reaction-times to the pairs, it is seen by reference to the table that the decreases from the pairs of stimuli are also quantitatively near each other. This lends further support to the opinion that it is not possible to select out of a group of simultaneous stimuli one of its members as a primary stimulus for the reaction movement. These facts are shown again even more plainly by converting these absolute differences into per cents of the single and paired members of the groups of simultaneous stimuli (Table XIV.). The mean variation of the single decrease per cents from their average is 1.3σ.

TABLE XIII

SUBJECT *P*. COMPARISON OF REACTION-TIMES TO GROUPED STIMULI WITH REACTION-TIMES TO SINGLE AND PAIRED STIMULI

Reaction to	Amounts of Decrease from								
LSE	*L* 52.1	*S* 17.1	*E* 13.7	*LS* 15.3	*LE* 4.8	*SE* −2.7	*SL* 12.9	*EL* 13.2	*ES* 1.7
P.E.	*1.5*	*3.6*	*2.0*	*1.8*	*1.3*	*1.8*	*2.3*	*3.0*	*1.1*
ELS	51.7	16.7	13.3	14.9	4.4	−3.1	12.5	12.8	1.3
P.E.	*1.7*	*3.7*	*2.2*	*1.9*	*1.5*	*1.9*	*2.4*	*3.1*	*1.4*
SLE	55.0	20.0	16.6	18.2	7.7	.2	15.8	16.1	4.6
P.E.	*1.7*	*3.7*	*2.2*	*2.0*	*1.6*	*1.9*	*2.5*	*3.1*	*1.4*

TABLE XIV

Subject *P*. Amounts of Decrease of Table XIII. Converted into Per Cents

Percents of Decrease from

LSE	*L* 30.3	*S* 12.4	*E* 10.2	*LS* 11.3	*LE* 3.8	*SE* −2.3	*SL* 9.7	*EL* 9.2	*ES* 1.4
ELS	30.1	12.2	9.9	11.0	3.5	−2.6	9.4	9.6	1.1
SLE	32.0	14.6	12.4	13.4	6.1		11.9	12.1	3.7

As would be expected in the case of subject *P* whose heightened sensitivity to the electric shock stimulus has been noted before, Table XV. shows that it displaces the sound stimulus from its position of greatest facilitation effect in Table IX. and causes it to be classed as next greatest in facilitation effect. Light holds its former position in this table by showing the least facilitation effect.

TABLE XV

Subject *P*. The Relative Facilitation Efects of Light, Electric Shock and Sound

Fastest with Slowest				Medium with Slowest		
L	*LS*	*d*	Per Cent.	*LE*	*d*	Per Cent.
171.7	134.9	36.8	21.4	124.4	47.3	27.5
	P.E.	*2.1*			*1.6*	
Fastest with Medium				**Slowest with Medium**		
E	*ES*					
133.3	121.3	12.0	9.0	132.8	.5	.4
	P.E.	*2.0*			*3.4*	
Medium with Fastest				**Slowest with Fastest**		
S	*SE*			*SL*		
136.7	116.9	19.8	14.4	132.5	4.2	3.1
	P.E	*2.4*			*3.7*	

The following table and its graphic representation (Fig. 5) show the comparison between the reaction-times of subject *P* to the pairs of simultaneous stimuli when their different members were in turn designated for reaction attention. It will be seen that they are quantitatively close together, and fall near each other in the graph. The reaction-time to *ELS*, or 120.0σ, not given in the table, is shown on the graph.

Subject *P*. Comparison of Reaction-times to Pair and Group Members.

LS	139.4	*LE*	124.4	*SE*	116.9	*LSE*	119.6
SL	132.5	*EL*	132.8	*ES*	121.3	*SLE*	116.7
d	6.9		8.4		4.4		2.9
P.E.	*2.7*		*3.1*		*3.0*		*1.4*

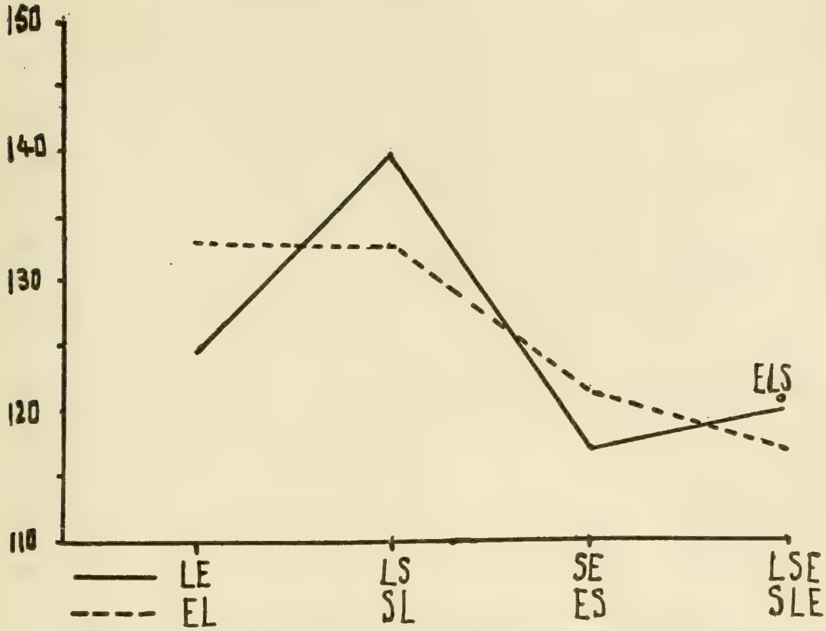

Fig. 5. Subject *P*. Reaction-times to Pair and Group Members.

V.

Reaction to Simultaneous Stimuli after Correction for the Latent Periods of the Sound-hammer and Induction Coil

After correction had been made for the sound hammer distance the stimuli were not yet, strictly speaking, simultaneous, for the latent periods of the sound hammer magnets and of the induction coil had not been taken into consideration. As the primary circuit-breaking apparatus was adjusted to break the circuit in the instant the light appeared in the aperture before the reagent's eye, whatever latency there was in the induction coil would cause the electric stimulus to be given at the reagent's finger just that much later than the light appeared. It was estimated by an electrical engineer that the latency of the coil employed in this experiment was from 6 to 8σ. It was not necessary to measure this latent period as it could be eliminated as far as all practical needs were concerned, by placing an accumulator or condenser across the primary circuit. This decreased the spark incident upon the break of the circuit to one that was noticeable only on close observation, and made the break of the primary circuit more sudden than before, and produced the induced current to all practical purposes instantaneously. The loss of time in the inductorium is not due to the coil but to the spark that follows the break of the primary circuit. The use of the condenser is effective only in cases where an induction coil with a laminated core is employed. Thus, the introduction of the condenser into the primary circuit of the induction coil enables, as far as it is possible, a simultaneous presentation of two of the stimuli, light and electric shock.

It was necessary, however, to determine the latency of the sound hammer in sigma units and to make corrections for the time as the hammer closed the gravity battery circuit and threw the current into the chronoscope. Whatever latency there was in the sound hammer magnets, after correction for the sound hammer distance, represented the tardiness of the sound among the stimuli, so-called simultaneous, and a like delay in setting the clock-work of the chronoscope into motion.

To determine the latent period of the electro-magnets of the sound hammer, the hammer was mounted upon the carriage of a

Schumann Horizontal Chronograph, after removing the magnetic marker, in such a manner that the plane of the median line of the hammer and the arm of the hammer were tangent to the drum of the chronograph. It was easy, then, by attaching a rigid marker to the end of the bar or arm which bore the hammer, and adjusting it to come into contact with the smoked paper of the chronograph drum, to record on the smoked paper the movement of the hammer after the circuit to its electro-magnets had been closed. This provided for marking the conclusion of the period of latency of the magnets. It was necessary to devise a way for marking the instant in which the circuit to the magnets was closed which would represent the beginning of the latent period to be measured. The period of latency could then be calculated from the interval between the circuit-closing event and the hammer response, both being marked upon the smoked paper of the drum by an offset in their respective lines.

To record the instant in which the sound hammer circuit was closed, a Morse key was mounted upon the base of the sound hammer so that the horizontal plane of its lever was also tangent to the surface of the chronograph drum. A rigid marker was fastened to the bar just beneath the button of the key and adjusted so as to touch the smoked paper of the drum. The points of the markers on the sound hammer bar and on the Morse key were carefully squared with each other and while at rest during the revolution of the chronograph drum made two parallel lines near together and close beside the record of the tuning fork. At each depression of the button of the Morse key there was produced an offset in the line made by its marker, and shortly afterward, an offset in the line made by the marker attached to the bar of the sound hammer. The offsets were not right-angled as the velocity of the chronograph drum threw them into obtuse angles. In measuring off the interval upon the time markings of the tuning fork, its beginning was counted from the extremity of the line produced by the depression of the Morse key to the beginning of the offset in the line produced by the marker attached to the lever bar of the sound hammer.

About seventy tests of this kind were made and their average was less than three vibrations of the 250-d.v. tuning fork of the chronograph. In this manner it was determined that the latent period of the electromagnets of the sound hammer was less than 12σ. Correction for this latency was made by advancing the sound hammer contact (*s*, Fig. 1) through a distance derived by multiplying the previous correction for the sound distance by 3, plus the distance of the previous correction. It was not possible to correct to the 1σ

with any feeling of certainty, but this was of no consequence, as a correction of 4σ previously made did not produce a determinable effect upon the reaction-times.

After these corrections the stimuli employed in this division were as nearly simultaneous as it was mechanically possible to get them. When the sound hammer and induction coil give their stimuli independent of each other and no attempt is made to produce them simultaneously, there is no necessity of knowing and correcting for their latent periods which in such cases give a constant error that affects only the absolute magnitude of the reaction-times and not their relative values. But need of knowing the latent periods of the sound hammer magnets and of the induction coil arises when the attempt is made to present the electric shock stimulus and the sound stimulus simultaneously.

TABLE XVI

Reaction to Single, Paired and Grouped Simultaneous Stimuli after Correction for Latency of Sound Hammer and Induction Coil

Subject	*M*		*P*		*T*		Gross	
	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
Light	168.0	*12.0*	175.6	*5.7*	185.8	*7.2*	176.4	*8.3*
LE	145.9	*10.1*	140.4	*9.6*	157.4	*7.9*	147.9	*9.2*
LS	131.3	*13.6*	136.9	*4.7*	160.1	*8.5*	142.7	*8.9*
LES	114.4	*4.8*	123.8	*4.8*	133.8	*9.1*	124.0	*6.2*
Electric ...	141.5	*13.6*	135.1	*8.6*	152.1	*9.6*	142.9	*10.6*
EL	129.9	*11.0*	132.7	*8.4*	144.6	*6.0*	135.7	*8.4*
ES	123.7	*13.5*	122.0	*7.5*	136.8	*7.8*	127.5	*8.4*
ELS	124.6	*14.5*	125.0	*4.9*	138.1	*11.2*	129.2	*10.2*
Sound	134.9	*9.3*	131.9	*5.7*	160.5	*7.3*	142.4	*7.4*
SL	135.5	*9.0*	133.5	*8.8*	157.3	*7.4*	142.1	*8.4*
SE	125.5	*7.4*	122.8	*4.9*	153.0	*8.6*	142.1	*6.9*
SLE	121.7	*14.8*	123.6	*7.0*	142.5	*6.3*	129.2	*9.3*

Each entry in Table XVI, represents the average of 100 reaction-times. The mean variations are calculated from the deviations of the average of each set of 10 single reactions from the average of a series of 10 sets amounting to 100 individual reaction-times. The table and its graphic representation, Fig. 6, show that there is a gradual decrease in the reaction-times as the stimuli pass from single stimuli to paired stimuli, and from paired to grouped simultaneous stimuli. Fig. 6 shows that *M* and *P*'s reaction-times are quantitatively close together, and that *T*'s reaction-times are distinctly higher than those of *M* and *P*, but have the same relative duration, save that for some unknown cause and contrary to previously found relations, *T*'s time to the simultaneous group *LS* is in relation to the rest of his reaction-times, much longer than *M* and *P*'s.

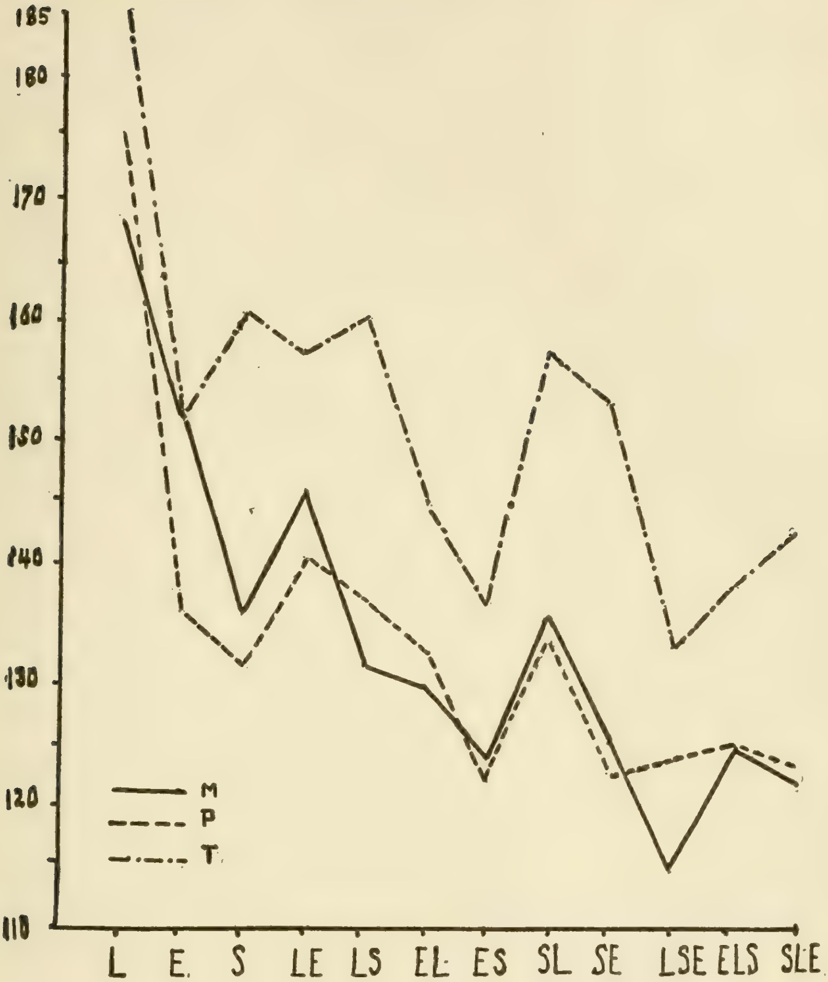

FIG. 6. Graphic Representation of Table XVI.

Table XVII. shows that for one of the subjects, *T,* the electric shock stimulus gives a shorter reaction-time than sound, contrary to what has been earlier found in this experiment and contrary to what has been usually found by other experimenters. The gross averages show scarcely any difference in favor of sound as the stimulus that produces the shortest reaction-time. The reaction-times to shock and sound seem closer together than usual for the three subjects. This may be due to the correction of about 12σ for the latency of the electro-magnets of the sound hammer which would tend to increase by that amount the reaction-times to the sound

stimulus by setting the chronoscope in motion that much earlier than before.

TABLE XVII

Reaction to the Single Stimuli

	Light		Shock		Sound	
	Av.	M.V.	Av.	M.V.	Av.	M.V.
M	168.0	*12.0*	141.5	*13.6*	134.9	*9.3*
P	175.6	*5.7*	135.1	*13.6*	131.9	*5.7*
T	185.8	*7.2*	152.1	*9.6*	160.5	*7.3*
Av.	**176.4**	*8.3*	**142.9**	*12.2*	**142.4**	*7.4*

Tables XVIII. and XIX. show that with a few exceptions the reaction-times to the pairs of simultaneous stimuli are shorter than the individual reaction-times to the members of which they are made up. The averages of the decreases of the reaction-times to the pairs of simultaneous stimuli are again averaged and given in Table XX. Each of the entries of this table is derived from 300 single reaction-times.

TABLE XIX

The Decreases of Table XVIII. Converted into Per Cents

Subject		*M*	*P*	*T*		*M*	*P*	*T*
LS from	*L*	21.8	22.0	13.8	*S*	2.6	—3.7	.2
SL		19.3	23.9	14.8		—.4	—1.2	1.9
LE	*L*	13.2	19.9	14.7	*E*	—3.1	—3.9	—3.4
EL		22.7	24.4	22.1		8.2	1.8	4.9
SE	*S*	6.9	6.9	4.6	*E*	11.3	9.1	—.5
ES		8.3	7.5	14.7		12.5	9.6	10.1

TABLE XX

Average Per Cent. of Decrease of Reaction-times to Pairs from Reaction-times to Their Individual Members

LS from *L*	12.4	*S*	—.3	*LE* from *L*	15.9	*E*	—3.4	*SE* from *S*	6.1	*E*	6.6
SL	19.3		.1	*EL*	23.1		4.9	*ES*	10.1		10.7

Tables XXI. and XXII. for reagents *M*, *P* and *T* support the previous finding in the case of reagents *A*, *P* and *T*, namely, that the reaction-times to a group of three simultaneous stimuli are less than the reaction-times to the members of the group when presented alone, and less than the reaction-times to the pairs of stimuli into which it is possible to arrange the individual members of each group.

Deuchler[1] gives tables showing that reaction-times to compound stimuli are on the whole shorter than the reactions to each component member.

[1] "Beiträge zur Erforschung der Reaktionsformen," *Psychol. Stud.*, **5**, 11 seq., 1909–10.

TABLE XVIII

COMPARISON OF REACTION-TIMES TO PAIRED STIMULI WITH REACTION-TIMES TO SINGLE STIMULI

	Subject *M*						Subject *P*						Subject *T*					
	Av.	M.V.	Decrease from				Av.	M.V.	Decrease from				Av.	M.V.	Decrease from			
L	168.0	*12.0*					175.6	*5.7*					185.8	7.2				
E	141.5	*13.6*					135.1	*8.6*					152.1	*9.6*				
S	134.9	*9.3*					131.9	*5.7*					160.5	*7.3*				
LS	131.3	*13.6*	*L*	36.7	*S*	3.6	136.9	*4.7*	*L*	38.7	*S*	—5.0	160.1	*8.5*	*L*	25.7	*S*	.4
		P.E.		*4.7*		*4.3*				*1.9*		*1.7*				*2.9*		*2.9*
SL	135.5	*9.0*		32.5		—.6	133.5	8.8		42.1		—1.6	157.3	*7.4*		28.5		1.9
		P.E.		*4.0*		*3.4*				*2.8*		*2.8*				*2.7*		*2.7*
LE	145.9	*10.1*		36.7	*E*	3.6	140.4	*9.6*		35.2	*E*	—5.3	157.4	7.9		14.7	*E*	—3.4
		P.E.		*4.1*		*4.4*				*3.0*		*3.5*				*2.8*		*3.3*
EL	129.9	*11.0*		38.1		11.6	132.7	*8.4*		42.9		2.4	144.6	*6.0*		22.1		4.9
		P.E.		*4.3*		*3.8*				*2.7*		*3.2*				*2.4*		*3.0*
SE	125.5	*7.4*	*S*	9.4	*E*	16.0	122.8	*4.9*	*S*	9.1	*E*	12.3	153.0	*8.6*	*S*	4.6	*E*	—.5
		P.E.		*3.2*		*4.1*				*1.9*		*2.6*				*3.0*		*3.4*
ES	123.7	*13.3*		11.2		17.8	122.0	7.5		9.9		13.1	136.8	7.8		14.7		10.1
		P.E.		*4.2*		*5.1*				*2.5*		*2.4*				*3.0*		*2.8*

TABLE XXI

Decrease of Reaction-times to Grouped Stimuli from the Reaction-times to Their Individual Members

	Stimuli		Amounts of Decrease from								
	LSE										
	Av.	M.V.	L	E	S	LE	LS	EL	ES	SL	SE
M	114.4	4.8	53.6	27.1	20.5	31.5	16.9	15.5	9.3	21.1	11.1
		P.E.	3.4	3.8	2.8	2.9	3.9	3.2	3.7	2.7	2.3
P	123.8	4.8	51.8	11.3	8.1	16.6	13.1	8.9	—1.8	9.7	—1.0
		P.E.	1.9	2.4	2.0	2.9	5.7	2.6	2.4	2.7	1.8
T	133.8	9.1	52.0	18.3	26.7	23.6	26.3	10.8	3.0	23.5	19.2
		P.E.	2.5	3.1	2.5	2.7	2.9	2.3	2.7	2.6	2.8
Av.	**124.0**	6.2	**52.4**	**18.9**	**15.1**	**23.9**	**18.7**	**11.7**	**3.5**	**18.1**	**9.7**
		P.E.	1.6	1.7	1.5	1.9	1.7	1.6	1.9	1.6	1.4
	LSE										
M	124.6	14.5	43.4	16.9	10.3	21.3	6.7	5.3	—.9	10.9	.9
		P.E.	5.1	5.3	4.6	4.7	5.3	4.8	5.2	4.5	4.2
P	125.0	4.9	50.6	10.1	6.9	15.4	11.9	7.7	—3.0	8.5	—2.2
		P.E.	2.0	2.6	2.0	2.9	1.8	2.6	2.4	2.3	1.8
T	138.1	11.2	47.7	14.0	22.4	19.3	22.0	6.5	—1.3	19.2	14.9
		P.E.	3.5	3.9	3.6	3.6	3.7	3.3	3.6	3.6	3.7
Av.	**129.2**	10.2	**47.2**	**13.6**	**13.2**	**18.6**	**13.5**	**6.5**	**—1.7**	**12.8**	**4.5**
		P.E.	2.0	2.1	1.9	2.1	2.1	2.0	2.0	2.0	1.9
	SLE										
M	121.7	14.8	46.3	19.8	13.2	24.2	9.6	8.2	2.0	13.8	3.8
		P.E.	5.1	5.3	4.7	4.8	5.4	4.9	5.3	4.6	4.3
P	123.6	7.0	52.0	11.5	8.3	16.8	13.3	9.1	—1.6	9.9	—.8
		P.E.	2.4	2.9	2.4	3.2	2.2	2.9	2.7	2.9	2.2
T	142.5	6.3	43.3	9.6	18.0	14.9	17.6	2.1	—5.7	14.8	10.5
		P.E.	2.5	3.1	2.6	2.7	2.8	2.3	2.7.	2.6	2.8
Av.	**129.2**	6.3	**43.3**	**9.6**	**18.0**	**14.9**	**17.6**	**2.1**	**—5.7**	**14.8**	**10.5**
		P.E.	1.6	1.7	1.7	1.7	1.7	1.6	1.6	1.6	1.4

TABLE XXII

Per Cents of Decrease of Reaction-times to Grouped Stimuli from the Reaction-times to Their Individual Members

LSE	L	E	S	LE	LS	EL	ES	SL	SE
M	31.9	19.8	15.1	21.5	12.8	11.9	7.5	15.7	8.8
P	29.5	8.3	6.1	11.8	9.5	6.6	—1.4	7.2	—0.8
T	27.9	12.0	16.6	14.9	19.6	7.4	2.2	14.9	12.5
Av.	**29.7**	**13.3**	**12.6**	**16.1**	**13.9**	**8.6**	**2.7**	**12.6**	**6.8**
ELS									
M	25.8	11.9	7.6	14.6	5.1	4.1	—0.7	8.0	0.8
P	28.8	7.5	5.2	10.9	8.6	5.8	—2.4	6.3	1.8
T	25.6	9.2	13.9	12.2	13.7	4.5	—0.9	12.2	9.6
Av.	**26.7**	**9.5**	**8.9**	**12.5**	**9.1**	**4.8**	**—1.3**	**6.4**	**4.0**
SLE									
M	27.5	13.9	9.7	16.5	7.3	6.3	1.6	10.2	3.0
P	29.1	8.5	6.3	11.9	9.7	6.8	—1.3	7.4	—0.6
T	23.3	6.3	11.2	9.5	10.9	1.4	—4.2	9.4	6.8
Av.	**26.6**	**9.6**	**9.1**	**12.9**	**9.3**	**4.8**	**—1.5**	**9.0**	**1.1**

The table given below is computed from the data of Dunlap and Wells.[2] Each entry represents the average of the number of reactions indicated, and the gross averages represent the average reaction-times of the reagents *J, M, W* and *D* to the different stimuli, namely, sound alone; light alone; simultaneous sound and light with designated reaction to the sound; and simultaneous light and sound with designated reaction to the light. These data show that the reaction-time to sound was less than the reaction-time to light and agrees with the present experiment in the general relation existing between the two, but differs with respect to the brevity of the reaction-times to sound. It was found in the present experiment that none of the average reaction-times to sound for the different subjects was shorter than 130σ. The average reaction-times of Dunlap and Wells' subjects *J* and *M* respectively were very low, being 107.1 and 97.8σ. Breitwieser[3] also found very short times for the stimuli he used but accounts for the fact by a constant error in the working of the apparatus he employed.

The data computed from the tables of Dunlap and Wells show also that the reaction-time to simultaneous light and sound with instruction to the reagents to react to the sound (*Sf*, where *f* means light) was slightly greater on the average, than the reaction-time to sound alone, but distinctly less than the reaction-time to light alone. This is contrary to what was found in the present experiment where the reaction-time to simultaneous sound and light with instruction to react to the sound was, as has been shown, less than the reaction-time to either sound or light presented alone. The data show also that the reaction-time to simultaneous sound and light with instruction to react to the light is greater than the reaction-time to sound alone, and less than the reaction-time to light presented alone. This is again contrary to what has been found in the present experiment where the reaction to simultaneous sound and light with instruction to react to the light gave reaction-times which on the average were less than the average reaction-time to sound and light when presented alone. The data above quoted show that the reaction-time to light was longer than either the reaction-time to *Sf* or to *Fs* (where *f* means light, and each capital letter is the initial of the designated stimulus). Finally the data show that the reaction-times to *Sf* are less than the reaction-times to *Fs*. But the experimenters do not think that this means that the reagents in the case of simultaneous *Sf* singled out successfully the sound for reaction, thereby naturally showing shorter reaction-times, although

[2] "Reactions to Visual and Auditory Stimuli," *Psychol. Rev.*, **17**, 325, 1910.

[3] "Attention and Movement in Reaction-time," ARCHIVES OF PSYCHOLOGY, **18**, pp. 4–5, 1911.

the data seem to indicate this possibility. They made a test elsewhere quoted in this article which seemed to show that the reagents could not successfully single out of a pair of simultaneous stimuli one of its members as the object of the reaction process. In the present experiment, as already shown, there was found no clear evidence in favor of the reagents' power to do this.

TABLE FROM THE DATA OF DUNLAP AND WELLS

Stimulus:	*S*	*F*	*Sf*	*Fs*
No. of *r–ts.*	*129*	*125*	*128*	*128*
Sub *J*, Av.	107.1	155.7	107.0	113.8
	100	*100*	*100*	*100*
M	97.8	157.6	94.3	120.4
	90	*90*	*90*	*90*
W	119.5	175.3	123.3	144.2
	100	*101*	*100*	*100*
D	106.0	166.6	112.2	119.5
Gen. Av.	**107.6**	**163.5**	**109.2**	**124.5**

The reagents of Dunlap and Wells reported that in the cases of *Sf* and *Fs* their attention was almost without exception to the indicated stimulus, the other seeming to be noticed only after the reaction. All reported that the reactions to light seemed much slower than the reaction to sound. The reaction to sound seemed to follow directly upon the stimulus, as viewed in retrospect, while there seemed to be a slight pause or hesitation in the case of reaction to light. This introspection agrees with that obtained in the present experiment, and is discussed in a later division of this paper.

TABLE XXIII

THE RELATIVE FACILITATION EFFECTS OF LIGHT, SHOCK AND SOUND

	Fastest with Slowest				Medium with Slowest				
	L	*LS*	d_1	Per Cent.	*LE*	d_2	Per Cent.	$d_1—d_2$	Per Cent. difference
M	168.0	131.3	36.7	21.8	145.9	22.1	13.1	14.6	8.7
		P.E.	*4.7*			*4.1*			
P	175.6	136.9	38.7	22.0	140.4	35.2	20.0	3.5	2.0
		P.E.	*1.9*			*3.0*			
T	185.8	160.1	25.7	13.8	157.4	28.4	15.2	2.7	—1.4
		P.E.	*2.9*			*2.8*			
Av.	**176.4**	**142.7**	**33.7**	**19.2**	**147.9**	**28.5**	**16.1**	**5.2**	**3.1**
		P.E.	*1.9*			*1.9*			
	Fastest with Medium				**Slowest with Medium**				
	E	*ES*	d_1	Per Cent.	*EL*	d_2	Per Cent.		
M	141.5	123.7	17.8	12.5	129.9	11.6	8.1	6.2	4.4
		P.E.	*4.1*			*3.8*			
P	135.1	122.0	13.1	9.7	132.7	2.4	1.7	11.7	7.0
		P.E.	*2.4*			*3.8*			
T	152.1	136.8	15.5	10.2	144.6	7.5	4.9	8.0	5.3
		P.E.	*3.1*			*2.4*			
Av.	**142.9**	**127.5**	**15.4**	**10.0**	**135.7**	**7.1**	**4.9**	**8.3**	**5.1**
		P.E.	*1.9*			*1.9*			

	Medium with Fastest				Slowest with Fastest				
	S	*SE*	d_1	Per Cent.	*SL*	d_2	Per Cent.		
M	134.9	125.5	9.4	6.9	135.5	—.6	—.4	10.0	7.3
		P.E.	*4.1*			*3.4*			
P	131.9	122.8	9.1	6.9	133.5	—1.6	—1.2	10.7	8.1
		P.E.	*1.9*			*2.8*			
T	160.5	153.0	7.5	4.6	157.3	3.2	1.9	4.3	2.7
		P.E.	*3.2*			*2.8*			
Av.	**142.4**	**133.7**	**8.6**	**6.1**	**142.1**	**.3**	**.1**	**8.5**	**6.0**
		P.E.	*1.5*			*1.7*			

Table XXIII. agrees with the tables in the other divisions which are concerned with the relative facilitation effects of light, electric shock and sound, by showing that light has the least facilitation effect or brings about the least reduction in reaction time when presented simultaneously with another stimulus. The table shows also that sound brings about the greatest reduction, and that electric shock by a very small margin below sound produces the medium facilitation effect. The closeness of the electric shock in this case to sound in the matter of facilitation effect produced has been explained in connection with some of the preceding tables where it was pointed out that the reagents became unusually responsive to the electric stimulus, thus introducing a difficulty that held out through the series of experiments which this division reports.

Generalized, the meaning of this table is that when a given stimulus is added to another or to a pair of other and different stimuli there is a reduction in the reaction-time that varies with the independent reaction-time of the stimulus added; that is to say, a stimulus which produces the longest independent reaction-time when added to one or to two other disparate stimuli causes a reduction that is less than would be caused by the addition of one whose independent reaction-time was faster. The stimulus whose independent reaction-time is shortest causes the greatest reduction in the reaction-time when added to another stimulus or to two other disparate stimuli.

TABLE XXIV

Comparison of Reaction-times to the Same Groups

	LS	*SL*	*LE*	*EL*	*SE*	*ES*	*LES*	*ELS*	*SLE*
M	131.3	135.5	145.9	129.9	125.5	123.7	114.4	124.6	121.7
P	136.9	133.5	140.4	132.7	122.8	122.0	123.8	125.0	123.6
T	160.1	157.3	157.4	144.6	153.0	136.8	133.8	138.1	142.5
Av.	**142.7**	**142.1**	**147.9**	**135.7**	**133.7**	**127.5**	**124.0**	**129.2**	**129.2**

Each entry is the average of 100 reaction-times, and each general average member an average of 300 reaction-times.

VI.

The Ability to React to a Designated Member of a Pair or Group of Simultaneous Stimuli

§ 1. *General Summary of Evidence in the Previous Experiments.*—At this point in the interpretation of the data there is an opportunity to get material of a somewhat exhaustive amount which would warrant a definite conclusion on the question as to the power of a reagent to single out of a pair or group of simultaneous stimuli one of them as the object of his reaction response. Taking the averages of the reactions of *A, P, T,* to the various pairs and groups when instructed to react to a certain member in the group, and similar reactions for *M, P* and *T,* and, the reaction-times of *P* alone which are given in Division IV. and averaging them we obtain quantities which represent 700 individual reactions. These results are shown in Table XXV., and by Fig. 7 which is a graph of the weighted averages. It seems rather conclusive in view of these facts that there is no clear difference revealed in the reaction-times between reactions made to the various members of groups of two and three simultaneous stimuli respectively.

TABLE XXV

General Summary of Reaction-times to Groups with Designated Stimuli

	LS	*SL*	*LE*	*EL*	*SE*	*ES*	*LSE*	*SLE*	*LSE*
Av. *A, P, T,*	144.0	148.6	152.9	161.4	151.8	142.5	131.6	133.9	139.5
M, P, T,	142.7	142.1	147.9	135.7	133.7	127.5	124.0	129.2	129.2
P,	139.4	132.5	124.4	132.8	116.9	121.3	119.6	116.7	120.0
Gen. Av.	*142.0*	*141.1*	*141.7*	*143.3*	*134.1*	*130.4*	*125.1*	*126.6*	*129.5*
Weighted Av.	**142.8**	**143.5**	**146.7**	**146.3**	**139.0**	**133.0**	**126.6**	**129.4**	**132.6**

Each average quantity represents 700 reaction-times.

§ 2. *A Special Experiment.*—In this test the different pairs and groups of simultaneous stimuli were presented to the reagents under two different instructions. In one case they were to single out as the special reaction motive a designated member of the pair or group of simultaneous stimuli, and in the other case the instruction was to react to the group as a whole with no effort to attend to any of the elements of the pairs or groups. A series of five sets of 10 single reactions was run for each member of a pair or group of stimuli, and for each group, that is, 50 reactions to a designated member of a pair or group and 50 in each case to the group as a

unit. These two ways of presenting the stimuli were alternated and shifted so that the reaction was not regularly to the member of the group or to the group as a unit.

In Table XXVI. each entry is an average of 50 single reaction-times. The mean variations are obtained by averaging the deviations of the averages of each set of 10 reaction-times from the

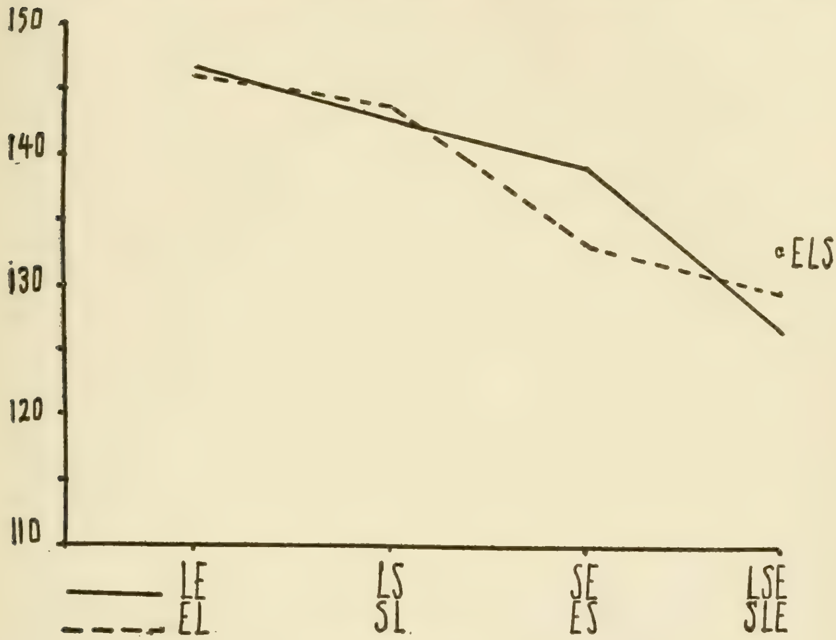

FIG. 7. Graphic Representation of Table XXV.

average of the 50 reaction-times. This table and its graphic form, Fig. 8, seem to indicate that the reagents in this experiment did not successfully single out for reaction a given member of a simultaneous pair or group of stimuli. The reaction-times to the same pair or group seem to remain quantitatively near together on the whole, regardless of whether the instruction was for reaction to a given member or to the pair or group as a whole.

Dunlap and Wells[1] made a test of this power to single out of a group one of its members for reaction by requiring the discrimination of light from sound when there was reaction to one stimulus and not to the other, and the discrimination of the combination of the two from either separately, when there was reaction to the

[1] "Some Experiments with Reaction to Visual and Auditory Stimuli," *Psychol. Rev.*, 17, 319, 1910.

TABLE XXVI

REACTION-TIMES TO A GROUP AND TO ONE OF ITS MEMBERS SINGLED OUT FOR REACTION

	Subject T Memb. Av.		Group	M.V. A.V.	d	Subject P Memb.		Group		d
LS	169.6	*7.9*	169.4	*4.8*	.2	134.8	*5.2*	139.1	*4.5*	4.3
					P. E. *3.5*					P. E. *2.8*
LE	158.9	*8.3*	166.1	*5.2*	7.2	132.2	*1.4*	130.0	*2.1*	2.2
					P. E. *3.6*					P. E. *1.7*
LSE	153.3	*6.3*	153.3	*6.3*	.0	123.9	*4.0*	123.3	*1.3*	.6
					P. E. *3.2*					P. E. *2.1*
EL	158.3	*5.1*	161.9	*8.8*	3.6	128.7	*5.5*	128.0	*2.3*	.7
					P. E. *2.7*					P. E. *2.5*
ES	161.0	*7.8*	161.6	*10.9*	.6	129.5	*3.9*	120.3	*1.9*	9.2
					P. E. *4.0*					P. E. *2.2*
ELS	163.6	*11.6*	152.8	*8.0*	10.8	124.6	*1.2*	123.0	*2.8*	1.6
					P. E. *4.1*					P. E. *1.8*
SL	162.5	*2.2*	163.3	*3.5*	.8	137.1	*7.9*	141.3	*2.1*	4.2
					P. E. *2.2*					P. E. *2.8*
SE	150.0	*3.4*	145.7	*9.7*	4.3	128.9	*2.7*	128.1	*2.8*	8
					P. E. *2.9*					P. E. *2.1*
SLE	158.8	*7.6*	152.9	*7.1*	5.9	125.2	*1.8*	129.6	*10.8*	4.4
					P. E. *3.5*					P. E. *3.5*

combination and not to the single stimulus, and also the discrimination of light or sound alone from the combination of the two when there was reaction to the single stimulus and not to the combination. They concluded from their experiment that a single stimulus can not be singled out of a group of simultaneous stimuli.

If the findings of this division are true they are significant in showing the reflex or automatic nature of the reaction process and as an indication of the minimal part played by consciousness in the reaction movement. Consciousness seems to be active in the fore-period, the period of preparation for the reaction movement, and after this getting ready or taking a "set" for the stimulus has occurred a number of times the part that consciousness seems to play even in the fore-period appears very small. The reagents of this experiment reported to the writer that there were times when they suddenly became aware of the fact that they had for some time been getting ready and regularly reacting without having been conscious of it.

For the most part, the testimony of the reagents in the present experiment was that while reacting under instruction to single out a given member of a pair or group they felt very uncertain about their success, and felt a strong tendency to stop trying to select the stimulus designated. It seemed to them that the whole group was causing the reaction event. The reagents also stated that they

could get no different feeling toward the stimuli when instructed to react to a given member than when presented with the instruction to react to them as a group—there was general preparedness for the stimuli that was the same in both cases. They testified also that lapses frequently occurred in which they assumed the seem-

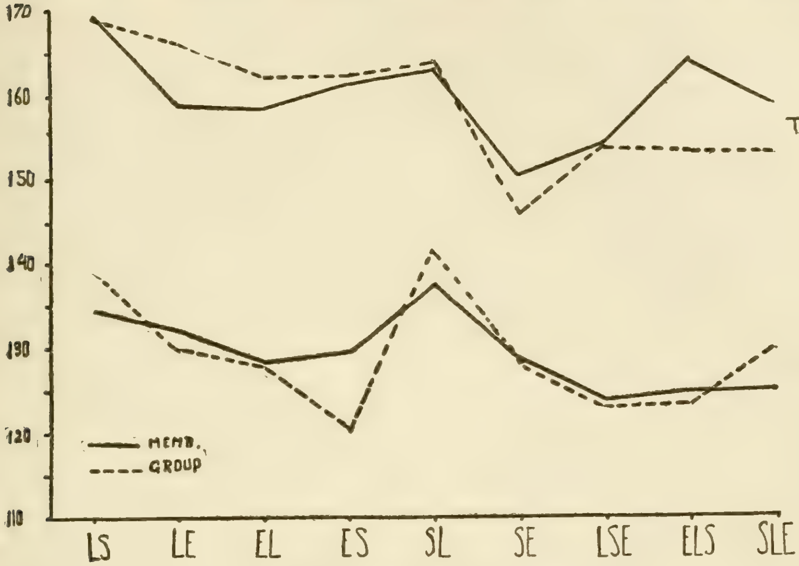

FIG. 8. Graphic Representation of Table XXVI.

ingly easier mode of reacting to the group. In several cases, *P*, after a number of very regular reactions, stated that he could not have told what the stimuli had been to which he reacted, which shows again the reflex nature of the process. The reaction-times obtained under these conditions were highly characteristic of the stimuli to which he had been reacting. Subject *T* was often not able to pass judgment upon the length of his reaction-times when they were the most automatic and reflex, but always knew the unusually short and long reaction-times. He felt the most uncertain about them when they were the best. It was noted above that the reaction-times were the best when the reaction process was the most automatic. All subjects stated that when they attended too closely to the reaction process they felt very uncertain about their reaction-times. The result of this feeling was that they made a number of tardy and premature reactions. The experimenter noticed this in the cases where the reagents were told to single out a member of a pair or group of stimuli. Subject *M* often emphatically stated that he was not able to single out a member as it disturbed him and made the

process disagreeable and far from what he desired it to be. In fact, the tendency on the part of the reagents was to fall into a cool, relaxed mood, as soon as possible, which enabled them to react better than an attitude of strained attention would have allowed. The reagents often judged their reactions valid because they felt at ease while reacting.

The question of attention in reaction-time has been widely discussed. Cattell[2] writes about this point as follows: " . . . the normal reaction-time of an observer can often be lengthened by directing him to fix his attention on the sense-impression, but it does not seem so evident that it can be shortened by directing him to fix his attention on the movement. The reaction-time is naturally lengthened and made more irregular when its automatic nature is disturbed; and from the experiments made in the Leipsic laboratory, it would seem that attending exclusively to the sense-impression is more disturbing than attending exclusively to the movement. In daily life, however, the contrary holds; actions are executed more automatically when the attention is directed to the sense-impression —thus in throwing, catching or striking a ball, the more completely one can attend to the ball and forget the movement, the more efficient and quick is the movement."

[2] "Reaction-time," *Psychol. Rev.*, **2**, 200, 1895.

VII.

Reaction to Stimuli of Low Intensities Accompanied by Simultaneous Stimuli of Medium Intensities

The simultaneous stimuli of the preceding divisions were as was stated of what was called medium intensities. In this division the three disparate stimuli, light, electric shock, and sound, were presented as before in simultaneous pairs and groups. There was also in this division a designation of one of the stimuli of the pairs and groups for reaction, but the designated stimulus differed from the accompanying stimuli in being of a low intensity, whereas the latter were of the usual and so-called medium intensities.

§1. *The Production of the Low Intensities of the Stimuli.*—The low intensity of the light was obtained by inserting before the ground glass slide of the stimulus lamp (*L*, Fig. 1) a screen of translucent paper washed in an ink solution till it transmitted a light that was much reduced in intensity but easily perceptible. The slide was readily detachable to allow the use of the light as a medium stimulus with the other stimuli each at a time of low intensity according to the various courses of the experiment. As this light of very low intensity was easily affected by the change of illumination in the room, greater care than usual was taken to keep the light in the experimental room constant.

The low intensity of the sound stimulus was obtained by adjusting the hammer scale so as to allow the hammer to fall though a distance of about 2 mm. only. The sound thus produced could be heard above the sound of the chronoscope, and caused no strain of the reagent's attention.

The low intensity of the electric shock stimulus was obtained by moving the induction coil away from the core until a shock was found for each of the reagents, that was weak, but of an intensity strong enough to remain fairly constant during the sitting. The low intensity of the shock, however, occasionally caused difficulty, as there sometimes occurred a further weakening of the shock that interfered with the regularity of the reaction-times. This may have been due to adaptation on the part of the reagent, to fluctuations of the current, to irregularities in breaking the primary circuit and to the reagent's failure to keep the electrode in the reaction key evenly moistened with the saline solution.

Two subjects only, *P* and *T*, were experimented upon in this division. As there was not sufficient time to make the experiment elaborate, five series of 10 reaction-times each constituted a set, instead of ten series of 10 reaction-times each as in the previous divisions.

In Table XXVII. the small letters *l, s, e,* represent the above-described low intensities of the light, sound, and electric shock stimuli. Beside them are given the averages of 50 single reaction-

TABLE XXVII

Reaction-times to Weak Stimuli Accompanied by Simultaneous Stimuli of Medium Intensities

				P				
l	189.5	*8.9*	*s*	159.0	*7.8*	*e*	168.4	*7.1*
lS	150.5	*4.9*	*sL*	156.1	*5.9*	*eL*	155.6	*7.2*
lE	144.1	*4.9*	*sE*	146.2	*4.9*	*eS*	141.8	*5.2*
lSE	132.5	*6.3*	*sLE*	141.4	*4.0*	*eLS*	140.6	*3.0*
				T				
l	200.9	*5.1*	*s*	179.2	*7.4*	*e*	194.7	*3.6*
lS	180.0	*6.5*	*sL*	170.0	*8.2*	*eL*	177.0	*4.4*
lE	181.2	*6.6*	*sE*	182.1	*4.2*	*eS*	190.2	*10.1*
lSE	171.7	*2.5*	*sLE*	169.3	*6.5*	*eLS*	168.5	*6.0*
				Av.				
l	195.2	*7.0*	*s*	169.1	*7.6*	*e*	181.5	*5.3*
lS	165.2	*5.7*	*sL*	163.0	*7.0*	*eL*	166.3	*5.8*
lE	167.6	*5.7*	*sE*	164.1	*4.5*	*eS*	166.0	*7.6*
lSE	152.1	*4.4*	*sLE*	155.3	*5.2*	*eLS*	154.5	*4.5*

times when they were presented alone. These reaction-times serve as a basis for comparison with the reaction-times to the low intensities after the other stimuli are added to them in medium intensity, singly and in pairs. Each entry in the table represents the average of 50 individual reaction-times. The symbol *lS* means that the stimulus was the light of low intensity accompanied simultaneously by the sound of medium intensity. Likewise *lE* means light of low intensity with simultaneous accompaniment of electric shock of medium intensity; *lSE* means that the pair of medium sound and electric shock stimuli were simultaneously presented with low intensity of light. In every case, as was stated, the instruction to the reagent was to react to the stimulus of low intensity. Reference to Table XXVII. will show by means of the small letters toward what stimulus or element of a multiple stimulus the reaction attention was directed.

§2. *Method and Results.*—Table XXVII. shows an increase in reaction-time for the two reagents with a decrease in the intensities of the stimuli, agreeing with the quotations of an earlier division of

this work. The general relations among the reaction-times to the single stimuli of low intensity remain the same as they were in the case of the single stimuli of medium intensity, namely, light longest, sound shortest and electric shock medium.

The chief interest in the data, however, is in noting the facilitating effect of the stimuli of medium intensity when presented simultaneously with those of low intensity. The low intensities of the stimuli make the reaction movement introspectively slow and seemingly heavy, but the addition of the stimuli of medium intensities causes a felt relief and makes the process easier.

Tables XXVIII. and XXIX. of the decreases and per cents of decrease show the result of adding stimuli of medium intensities to those of weak intensities.

In Table XXVIII. under *R* are given the various combinations of the stimuli. The decrease of their reaction-times from that to the other single and combined stimuli is also given. For example, the reaction-time to *lS* for subject *P* gives a decrease of 39.0^{σ} from the reaction-time to *l* alone. The reaction-time to *lE* gives a decrease of 45.4^{σ} from the reaction-time to *l* alone, while the reaction-time to the simultaneous group *lSE* shows a decrease of 57.5^{σ} from *l*, 13.6^{σ} from the pair *lE*, and 18.0^{σ} from the pair *lS*. Table XXIX. is read in the same manner.

Generalizing from these tables of results, it can be said that when stimuli of medium intensities are added to other stimuli of low intensities, there is commonly a decrease in reaction-time. The addition of two stimuli of medium intensities to another stimulus of low intensity brings about a reduction in reaction-time that is greater than that produced by the addition of one such stimulus. This is to be expected as there is in each case an increase in the intensity of the stimulus compound, but a greater increase in the former case than in the latter.

Save that subject *P* shows an over-sensitivity to the electric shock stimulus, the influence of the added stimuli varies with the quickness or slowness of the reaction-times to them alone. Medium light, which causes the slowest reaction-times produces the least decrease; electric shock, which causes the next slowest reaction, produces a greater decrease than light; and sound, which causes the fastest reaction, produces the greatest reduction in reaction-time.

Something definite in the matter of the ability of reagents to single out a member of a compound stimulus for reaction should be shown by this division. As the designated member was contrasted in intensity with its co-stimuli, it should make the reagent's ability to choose a given member of a group very apparent. The following

comparisons taken from the table of averages under Table XXVII. seem to indicate no distinct ability as all differences are below the mean variation, and have a large probable error.

lS	165.2	*lE*	167.6	*sE*	164.1	*lSE*	152.1	*eLS*	154.5	*sLE*	155.3
sL	163.0	*eL*	166.3	*eS*	166.0	*sLE*	155.3	*lSE*	152.1	*eLS*	154.5
d	2.2		1.3		1.9		3.2		2.4		.8
P.E.	*2.4*		*2.1*		*2.3*		*1.8*		*1.6*		*1.8*

TABLE XXVIII

DECREASE IN THE REACTION-TIMES DUE TO ADDING TO STIMULI OF WEAK INTENSITIES OTHER STIMULI OF MEDIUM INTENSITIES

	Subject *P* Decrease from			Subject *T* Decrease from		
	l	*lE*	*lS*	*l*	*lE*	*lS*
lS	39.0			20.9		
P.E.	*1.2*			*1.0*		
lE	45.4			19.7		
P.E.	*1.2*			*1.0*		
lSE	57.5	13.6	18.0	29.2	10.5	8.3
P.E.	*1.3*	*.9*	*.9*	*.7*	*.8*	*.8*
	s	*sL*	*sE*	*s*	*sL*	*sE*
sL	2.9			9.2		
P.E.	*1.2*			*1.3*		
sE	13.8			—2.9		
P.E.	*1.1*			*1.0*		
sLE	17.6	14.7	4.8	9.9	.7	12.8
P.E.	*1.0*	*.8*	*.7*	*1.2*	*1.2*	*9.3*
	e	*eL*	*eS*	*e*	*eL*	*sE*
eL	12.8			17.7		
P.E.	*1.2*			*.7*		
eS	26.6			4.5		
P.E.	*1.1*			*1.3*		
eLS	27.8	15.0	1.2	26.2	8.5	21.7
P.E.	*.9*	*.9*	*.7*	*.8*	*.9*	*1.4*

TABLE XXIX

AVERAGE DECREASES AND PER CENTS OF DECREASE IN REACTION-TIMES OF SUBJECTS *P* AND *T* DUE TO ADDING TO STIMULI OF WEAK INTENSITIES OTHER STIMULI OF MEDIUM INTENSITIES

			Decrease and Per Cent. Decrease from			
	l	Per Cent.	*lE*	Per Cent.	*lS*	Per Cent.
lS	29.9	15.3				
lE	32.5	16.6				
lSE	43.3	22.1	12.0	7.1	13.1	7.9
	s	Per Cent.	*sL*	Per Cent.	*sE*	Per Cent.
sL	6.0	3.5				
sE	5.4	3.1				
sLE	13.7	8.1	7.7	4.7	8.8	5.3
	e	Per Cent.	*eL*	Per Cent.	*eS*	Per Cent.
eL	15.2	8.3				
eS	15.0	8.2				
eLS	27.0	14.8	11.7	7.0	11.4	6.8

VIII.

Reaction to Successive Stimuli with Graded Intervals

§ 1. *Apparatus and Stimuli.*—The stimuli of this division were the light, electric shock and sound of the so-called medium intensities. These stimuli were presented in succession to the reagents with four different intervals between them. In the succession they always appeared in the following order: sound first; electric shock second; and light third. The reaction was always to the light the position of which was constant and which was so arranged as to appear in the aperture in the screen at the instant the chronoscope brush (ch_3, Fig. 1) passed upon the brass strip and closed the chronoscope circuit. The adjustable slide (*ad.s.*, Fig. 1) which operated to break the primary circuit of the induction coil was set so as to produce the electric shock stimulus a period of time after the occurrence of the sound agreeing with the interval chosen. The interval between the sound and the electric shock was measured off by advancing the sound brush contact (*s*, Fig. 1) along a rod not shown in Fig. 1, and clamping it in its proper position. The intervals used were respectively 360, 180, 90 and 45^σ. These various intervals were calculated from the 360^σ-interval which was determined by taking $360 \times$ the sigma-units of one revolution of the large wheel (*W*, Fig. 1) divided by the circumference units (centimeters) of the circular path (W_1, Fig. 1). Half this distance represented the 180^σ-interval, and in like manner the 90- and 45^σ-intervals were obtained. The contacts were set with care, yet it is not possible to say that the intervals did not vary a few sigmas above or below their supposed durations.

§ 2. *Method and Results.*—The stimuli were presented in two different sequences only, namely, S–E–L_r and E–L_r, where S means sound, E, electric shock, and L_r, light to which the reaction was made. The succession is indicated by the dashes between the initials of the stimuli. These S–E–L_r and E–L_r stimuli (the latter being the first minus the sound stimulus) were presented with the four different intervals, 360, 180, 90 and 45^σ, and reaction was in all cases to L_r.

One hundred reactions were taken to each of the successions for each interval, alternating irregularly between reaction to L_r in the sequences S–E–L_r and E–L_r. As the intervals were difficult to measure and the contacts hard to change and relocate, each interval

was completed for the two subjects *P* and *T* before change was made for the next interval. In the course of the experiment a preliminary signal was given which was soon followed by the first of the successive stimuli, and later by the second and third separated by the time-intervals chosen.

The general results are shown in Table XXX., where each entry is the average of 100 individual reaction-times. The averages of the averages of *P* and *T*'s reaction-times are given in Table XXXI. where each entry therefore represents the average of 200 reaction-times to L_r for a given interval.

TABLE XXX

REACTION-TIMES TO SUCCESSIVE STIMULI WITH GRADED INTERVALS

Interval	Subject *P* S-E-L_r Av.	M.V.	E-L_r Av	M.V.	Subject *T* S-E-L_r Av.	M.V.	E-L_r Av.	M.V.
360σ	180.5	*5.3*	182.8	*3.3*	222.6	*12.3*	217.5	*8.1*
180σ	203.1	*18.3*	222.3	*6.9*	213.8	*18.9*	218.4	*5.7*
90σ	173.9	*7.7*	173.6	*7.4*	180.0	*8.3*	175.3	*9.0*
45σ	159.9	*7.8*	156.6	*11.2*	177.1	*7.6*	182.4	*9.0*

TABLE XXXI

GROSS AVERAGES OF REACTION-TIMES TO SUCCESSIVE STIMULI

Interval		S-E-L_r		E-L_r	
	360σ	201.5	*8.8*	195.1	*5.7*
	180	208.4	*18.6*	220.3	*6.3*
	90	176.9	*8.0*	174.4	*7.8*
	45	168.5	*7.7*	169.5	*10.1*

The third part of Table XXXII., which gives the average differences and per cents of difference of the reaction-times for subjects *P* and *T*, shows that there is an increase in the reaction-times to L_r in both the sequences S–E–L_r and E–L_r when the interval between them is reduced from 360^σ to 180^σ. This is not significant as it is due to a large increase in the reaction-times of subject *P*. The reaction-times of subject *T* show a slight decrease through the reduction of the interval from 360^σ to 180^σ in the case of the sequence S–E–L_r, and a very slight negative decrease in the case of the sequence E–L_r. The average difference table shows, also, that a reduction of the intervals between the S–E–L_r sequence from 180^σ to 90^σ produces a gross difference in reaction-times of 24.6^σ or 12.2 per cent., on the average, and for the sequence E–L_r, 25.7^σ, or 12.2

per cent. The table shows again that a reduction of the interval between the successive stimuli from 90^σ to 45^σ brings about a further decrease in the reaction-times to L_r in the case of both sequences. While the effect of reducing the interval from 360^σ to 180^σ is uncertain, it is significant that a reduction of the interval between the sequences S–E–L_r and E–L_r from 180^σ to 45^σ produces a total decrease in reaction-time of 57.6^σ and 56.3^σ, or 28.6 per cent. and 27.4 per cent., respectively.

The results show that in the case of three successive stimuli with an interval of 360^σ between the first and second, and the second

TABLE XXXII

Differences and Per Cents of Difference Due to Shortening the Interval between the Successive Stimuli

Subject *P*

S-E-L_r			*d*	Per Cent.	*E-L_r*			*d*	Per Cent.
From 360	to	180	—22.6	—12.5	From 360	to	180	—39.5	—21.1
		P. E.	*5.1*					*2.0*	
360		90	6.6	3.6	360		90	9.2	5.0
		P. E.	*2.5*					*2.2*	
360		45	20.6	11.3	360		45	26.2	14.3
		P. E.	*2.5*					*3.1*	

Subject *T*

S-E-L_r			*d*	Per Cent.	*E-L_r*			*d*	Per Cent.
From 360	to	180	8.8	3.9	From 360	to	180	—.9	—.4
		P. E.	*6.1*					*2.6*	
360		90	42.6	18.1	360		90	42.2	19.4
		P. E.	*3.9*					*3.1*	
360		45	45.5	20.4	360		45	35.1	16.1
		P. E.	*4.3*					*3.1*	

Average Differences and Per Cents of Difference of the Reaction-times for *P* and *T*

S-E-L_r			*d*	Per Cent.	*E-L_r*			*d*	Per Cent.
From 360	to	180	—6.9	—3.4	From 360	to	180	—20.2	—10.7
		P. E.	*5.5*					*2.2*	
360		90	24.6	12.2	360		90	25.7	12.2
		P. E.	*3.1*					*2.6*	
360		45	33.0	16.4	360		45	30.6	15.2
		P. E.	*3.1*					*3.1*	

and third stimuli of the sequence, or in other words, with an interval of 720^σ between the first and third, the influence of the first stimulus and of the second stimulus is to delay the response to L_r, which in this case is slower for both reagents than when L_r is presented alone. These stimuli may then be called partially inhibitory to the response

to L_r. It might be said that the first stimulus S produces a disturbance that during the 360^σ that elapse before E appears has partially passed away, but is again excited by E and has not passed off when the final stimulus L_r appears. Thus the reaction to L_r is partially inhibited by the inertia of the previous disturbance. The full motor discharge that is usually set off by L_r when presented alone has been to some extent drained off in the other disturbances, and the reaction-time is longer than was the case when L_r appeared alone. The inhibitory effect seems to be on the whole about the same in the case of the 180^σ interval as for the 360^σ interval, but a diminution of the interval to 90^σ and to 45^σ shows a decided decrease in the reaction-times to L_r, that seems analogous to the decrease caused by an increase of intensity of stimulus. With the reduction of the intervals, there goes in fact a concentration of the stimuli as is shown by the following scheme in which the stimuli are represented by their initial letters and the time-intervals by their distances apart.

	Stimuli	Average Reaction-time
Interval 360σ	*S*———*E*———*L*ᵣ	201.5σ
180	*S*——*E*——*L*ᵣ	208.4
90	*S*—*E*—*L*ᵣ	176.9
45	*S*–*E*–*L*ᵣ	168.5
360	*E*———*L*ᵣ	195.1
180	*E*——*L*ᵣ	220.3
90	*E*—*L*ᵣ	174.4
45	*E*–*L*ᵣ	169.5

The shortest interval used, namely, 45^σ, was approximately the interval determined by Exner[1] that would allow the two successive and disparate stimuli, shock and light, to be sensed as successive. An interval shorter than this so-called "smallest interval" of Exner would result in a fusion of the two sensations, which would then seem to be simultaneous.

The general statement, then, is warranted, that when one or more stimuli precede a given stimulus to which reaction is to be made, the reaction-time to that stimulus is increased, and that a gradual reduction of this interval causes a gradual decrease in the reaction-time which approaches that to simultaneous stimuli. Wundt[2] has shown that when a disturbing noise precedes a stimulus to which reaction is to be made, the reaction-time to it is always increased. Wundt states also in the same connection that when the disturbing noise occurs simultaneously with the stimulus there is no

[1] "Physiologie der Grosshirnrinde," *Hermann's Handb. d. Physiol.*, **2**, 262, 1879.

[2] *Physiol. Psychol.*, **2**, 355, 1887.

more variation in reaction-time than if the disturbance had come after the stimulus. In view of the present experiment one would expect a reduction in reaction-time when the noise occurs with the stimulus.

At the close of the present experiment the writer had a series of 30 presentations of the light stimulus run upon him in which at each of the experimenter's "Ready" signals he squeezed with as nearly as possible equal efforts a hand-dynamometer, in the left hand, and when the stimulus appeared (about 1.5 to 2.0 sec. later) reacted with his right hand. The average of the 30 reaction-times thus obtained was 221.2σ as against a reaction-time under regular conditions of about 182.0σ.[3] The motor discharge in a certain direction had been partially drained off into another.

We have found then that inhibition takes place when stimuli follow each other with intervals of 360–45σ. The interesting question arises as to how much greater than 360σ the interval could be and still show an inhibitory effect. If the interval is long enough there would be complete recovery from each stimulus before its successor would appear and for which the reagent would therefore be properly set. The situation would be the same as reacting to single stimuli. Speculation might lead one to believe that with an interval of anything beyond the most often preferred interval between the forewarning and the stimulus, namely, 2 sec., the stimuli would be felt as separate and distinct and no longer as successive. In one sense of the word all single stimuli are successive, but there is a set and a discharge for each one that causes a renewal of the situation over and over again. The question is how much longer should the interval between successive stimuli be than the interval here employed (a little over one third of a second), to produce the effect of reacting to L_r alone. In other words, what is the least interval between a sequence of stimuli that will allow the reagent to recover from the influence of each stimulus? Further experimentation alone can answer this.

The general introspection of the reagents was that as the stimuli were given nearer and nearer together, their effects seemed to summate, and the reaction-movement seemed more rapid and much easier.

It is well to call attention again to the fact that the reaction-times

[3] Lack of time did not permit the investigation of the effect upon the reaction-times of squeezing the dynamometer with the left hand at the same instant the stimulus appeared. This will be tried later. It would seem however that such procedure would result in a less increase of reaction-times than in the case above reported. On the other hand it is possible that this arrangement might reenforce the reaction-movement and cause a reduction in the reaction-times.

to the sequences $S–E–L_r$ and $E–L_r$ with intervals of 45^σ tend to approach but are much greater than the reaction-times to the simultaneous stimuli LSE and LE, showing plainly the inhibition still at work in this close interval.[4]

[4] It was planned to study the effect of three successive stimuli of the same kind upon the reaction-times. That is, three sucessive lights were to be presented with reaction designated to the third and in like manner three successive shocks and sounds. It was also planned to make a study of the positions of the stimuli in the sequences; that is, the sequences were to be presented in the following orders and their reaction-times compared: $S–E–L_r$, $E–S–L_r$; $L–E–S_r$, $E–L–S_r$; and $L–S–E_r$, $S–L–E_r$. A preliminary investigation of this question was made but was not elaborate enough to warrant deductions.

IX.

Reenforcement and Inhibition

§ 1. *The Reflex Nature of the Reaction Process.*—Cattell[1] was the first to point out the reflex nature of the reaction event, it having been previously thought that the stimulus was *perceived* and the reaction-movement *willed.* Wundt[2] held that in the case of the so-called sensory reaction perception was involved, while the motor reaction was a cerebral reflex. Cattell[3] agrees with Wundt that a muscular reaction is an acquired reflex, but is not convinced that an apperception process is called into play when the attention is directed

[1] "It has generally been assumed that the largest factors of the reaction-time are taken up by processes of perception and willing. I think however that if these processes are present at all they are very rudimentary. Perception and volition are due, we may assume, to changes in the cortex of the cerebrum, but reflex motions in answer to sense-stimuli, as in the contraction of the pupil and in winking, can be made after the cortex has been removed, and an animal in this condition can carry out motions adapted to the nature of the stimulus. If a pigeon from which the cerebral hemispheres have been removed is thrown into the air, it will not only fly, but also avoid obstacles and alight naturally on the ground. It seems to have consequently sensations of light but apparently no perceptions, either because it does not see color and form, or because it lacks the intelligence needed to understand their meaning. In the same way reactions such as we are considering can probably be made without need of the cortex, that is, without perception or willing. When a subject has had no practice in making reactions (in which case the reaction-time is usually longer than 150 σ), I think the will time precedes the occurrence of the stimulus. That is, the subject by a voluntary effort, the time taken up by which could be determined, puts the lines of communciation between the center for simple light sensations (in the optic thalami probably) and the center for the coordination of motions (in the corpora striata, perhaps, connected with the cerebellum) as well as the latter center, in a state of unstable equilibrium. When therefore a nervous impulse reaches the thalami it causes brain changes in two directions; an impulse moves along the cortex, and calls forth there a perception corresponding to the stimulus, while at the same time an impulse follows a line of small resistance to the center for coordination of motions and the proper nervous impulse, already prepared and waiting for the signal, is sent from the center to the muscle of the hand. When the reaction has often been made the entire cerebral process becomes automatic, the impulse of itself takes the well-travelled way to the motor center and releases the motor impulse." ("The Time Taken Up by Cerebral Operations," *Mind,* **11**, 232, 1886. See also, "Psychometrische Untersuchungen," *Phil. Stud.*, **3**, 321, 1886.)

[2] "Physiol. Psychol.," **2**, 309, 1893.

[3] "Aufmerksamkeit und Reaction," *Phil. Stud.,* **8**, 403, 1893.

to the sense impression or to the sense organ. Cattell gives a table[4] which shows that his own reaction-times are the same whether his attention was directed to the movement or to the sense-impression. He states further that the distinctions of Lange[5] between sensory and motor reactions can not have general value, as with reagents whose reactions come quick and regular, there seems to be no difference due to the direction of attention while with reagents whose reaction-times are longer and less regular there is a lengthening of the reaction-time when the attention is sensory and not motor. In an article later than those already quoted,[6] Cattell restates his view that the reaction event is a cerebral reflex and gives a schematic drawing of the central and other elements involved in the reaction process.

James[7] states that the reaction process exactly resembles any reflex action, and differs only in being a transient result of a previous cerebral condition and not a permanent result of organic growth as in the case of the natural reflexes. James says in another place:[8] "The preparation of the attention and volition; and the readiness of the hand to move the instant it shall come; the nervous tension in which the subject waits, are all conditions of the formation in him for the time being of a new path or arc of reflex discharge." He further states that the motor center is raised to a condition of "heightened irritability" by the expectant attention. Woodworth calls the preparation a setting or opening up of a particular pathway which gives the incoming impulse a quick passage to the motor nerves.[9]

In Division VI. of this work it was shown that the reagents did not single out a designated stimulus when reacting to a pair or group of simultaneous stimuli, but responded to the entire stimulus-situation. This failure adds further testimony to the reflex nature of the reaction response.

§ 2. *Reenforcement and Inhibition in Voluntary and Reflex Actions.*—Reenforcement or facilitation has been noted by many

[4] *Ibid.*, 405.

[5] "Neue Experimente über den Vorgange der einfachen Reaction auf Sinneseindrücke," *Phil. stud.*, 4, 499, 1888. Lange called the motor reaction a brain reflex that was like the lower reflexes except that it required a preceding will stimulus. This will act caused a certain amount of potential energy to be stored up in the subcortical centers, where it rested in unstable equilibrium, ready to be set off into one channel or another, but acting most smoothly to a designated stimulus.

[6] "The Reaction-time on Light and the Time of Perception," Norris and Oliver's *System of Diseases of the Eye*, 1, 537, 1897.

[7] "Principles of Psychology," 1, 91, 1910.

[8] *Ibid.*, 90.

[9] "Physiological Psychology," 485, 1911.

experimenters in the case of the various natural reflexes. Jendrassik[10] reported that there was a more forcible knee-jerk when the hands were clenched just before the ligamentum patellæ was struck. Lombard[11] gives a curve which shows that the effect of clinching the teeth just before the patella was struck was to reenforce the knee-jerk as was shown in increased height of jerk. Lombard reports various other experiments which he performed that showed reenforcement of the patellar reflex, namely, irritation of the skin, voluntary actions (clenching the hands and teeth, etc.), excitation of the attention, cerebral inactivity and sleep,[12] different forms of cerebral activity, *e. g.*, multiplication and exciting mental work, reenforcement by respiration, by asphyxia, music and dreams.

Féré[13] studied the reenforcing influence of sensorial stimulations on voluntary movements. He found that when a sensorial stimulation was given simultaneously with the pressure on a dynamometer a greater force of contraction was produced than when the sensorial stimulation was absent. On page 27 of the citation he gives a table which shows that a normal force exerted on the dynamometer was for the right hand 23 kil. and for the left 15. After having the fingers of the right hand passively flexed five times, the units of force were respectively 41 and 14. After five equal active movements, 45 and 20; after counting to 45, the dynamometer recorded respectively, 44 and 24; after trying to add mentally 366 and 374, it recorded 41 and 36. Féré studied also the effect of sounds and music on the muscular effort, and found that increase of loudness and pitch caused corresponding increases of muscular effort. Sad notes decreased the contraction power of the hand, and lively ones increased it. Running the tonic scale through an octave showed a gradually increasing reenforcing influence. Lights of different colors caused different increments of force, blue causing the smallest and red the greatest increment. Féré reports in another place an experiment in which he found that moving the legs and counting in a loud voice reenforced the fingers engaged in working a Mosso ergograph. More work could be done with the eyes open than closed, and more under the influence of red light than any other. Sounds both harmonious and discordant increased the power of working. Musk and ethereal odors, the taste of sugar, acetic acid, and sulphate of quinine reenforced the working finger. Tastes were more effective than odors, and

[10] "Beiträge zur Lehre von den Sehnenreflexen," *Deutsches Archiv f. Klin. Med.*, **33**, 177, 1883.

[11] "Reenforcement of the Knee-jerk," *Am. Jour. of Psychol.*, **1**, 18, 1887–8.

[12] See also, Noyes, "Peculiarities of the Knee-jerk," *Am. Jour. of Psychol.*, **4**, 333, 1891–2.

[13] "Sensation et Mouvement," 26–50, 1887.

cutaneous sensations—heat, cold, friction, etc., had an accelerating influence. This reenforcing influence, called by Féré *dynamogénie*, has acquired the name dynamogenesis.[14]

This dynamogenic effect of concurrent stimuli upon voluntary movements and cerebral activity has been described by other experimenters. Hofbauer[15] found that firing a pistol caused a rise in the ergographic record. Whipple[16] shows a reenforcing power of enforced breathing on all activity.

Exner[17] found that a sound increased the amplitude of a reflex movement of the foot of a chloralized rabbit induced by a stimulus applied to the foot a moment later. Sherrington[18] points out that if in a spinal animal a forefoot and the hind foot on the crossed side are simultaneously stimulated the flexion movement of the hind leg is obtained more easily than when either is stimulated singly.

Cattell in an unpublished experiment tested the dynamogenic effect of contracting the left hand simultaneously with the right which recorded its force upon a dynamometer. The contractions were made at regular intervals so that at every fifth contraction of the right hand there was a simultaneous contraction of the left. The records showed that the right hand contracted more forcibly at every fifth trial. Bliss[19] found that the reaction-times to sound heard in two ears are shorter than the reaction-times for the same sound heard in one ear. Poffenberger[20] found that the reaction-times to a light seen with the two eyes are less than the reaction-times to the same light seen with one eye.

The present experiments have shown that whenever another stimulus was added to a given stimulus, or when a third stimulus was

[14] This term is defined (Baldwin, "Dictionary of Philosophy and Psychology," 302, 1901) as applying to the action of "any stimulus or influence which increases the available muscular power." It is defined by Ladd ("Psychology, Descriptive and Explanatory," 229, 1895: "Every sensation may be said to have a dynamogenetic value and influence in proportion to its intensity as well as to the way in which it fits in with the entire content of consciousness. [illegible] person is engaged in exerting pressure with a minimum of energy, any form of peripheral excitation may affect the potential of energy."

[15] "Interferenz zwischen Verschiedenen Impulsen in Centralnervensystem," *Pflüger's Archiv*, **68**, 546, 1897.

Am. Jour. Psych., **9**, 560, 1897–8.

[16] "The Influence of Forced Respiration on Psychical and Physical Activity," *Am. Jour. Psych.*, **9**, 560, 1897–8.

[17] *Pflüg. Archiv*, **28**, 487, 1882.

[18] "Intergrative Action of the Nervous System," 175, 1906.

[19] "Investigations in Reaction-time and Attention," *Yale Stud.*, **1–4**, 31, 1892–6.

[20] "Reaction-time to Retinal Stimulation," ARCHIVES OF PSYCHOLOGY, 23, 1912.

added to a pair of stimuli, there was a reduction in the reaction-time. That is to say, the added stimuli facilitated or reenforced the reaction-reflex. The subjects in the present experiment stated that when they reacted to the pairs or groups of stimuli the hand was lifted—seemed to bound—higher from the key and with more force than when reactions were being made to any of the stimuli alone. The facilitation thus subjectively felt was shown by reduced reaction-times. When reacting to light alone the finger seemed to lift heavily from the key, but when the other stimuli were added the key was released so suddenly as to produce a vibratory sound.[21] The subjects also found it much easier to keep in proper readiness for the stimuli when they were in simultaneous pairs or groups, and their reactions in such cases seemed to them more regular.

The more rapid responses to the simultaneous stimuli than to the single stimuli have been referred to in a previous division of this work as being due to a virtual increase in the extensity or intensity of the stimulus. Of this increase of intensity Sherrington[22] says: "The shorter latent times given by intenser stimuli seem readily explicable by the minimal quantity of transmitted influence necessary to give detectable effect, being necessarily earlier reached with copious transmission than with weaker transmission." The three simultaneous stimuli summate in excitatory effect and send their discharge down one common tract to the reacting finger. Woodworth[23] with Shäfer[24] and Sherrington[25] refer the time in the reflex event between the application of the stimulus and the response to a delay due to the passage of the nerve impulse across synapses. Woodworth says in another place:[26] "The conductibility is not as free and open across the synapse as across the parts of a continuous nerve. . . . It always retards the nervous impulse, or in other words, provokes a latent period of stimulation." Sherrington calls the synapse "a barrier whose resistance we do not exactly know." Strong stimuli, then, would force the barrier more quickly than weak ones, and the simultaneous attack of two or more stimuli would more rapidly send an impulse across than the single action of either of the stimuli.

[21] In 1909 in the psychological laboratory of Indiana University the writer performed an experiment in which the reagents were given a response word to be uttered when a certain other word was presented by a tachistoscope. It was found that whenever the stimulus word was the same as the response word there was much facilitation in the time of response.

[22] "Integrative Action of the Nervous System," 26, 1906.

[23] "Physiological Psychology," 479, 1911.

[24] "Textbook of Physiology," 2, 611, 1900.

[25] *Op. cit.*, 156.

[26] "Le Mouvement," 227, 1903.

In the case of the successive stimuli with an interval of 360^σ the reagents reported a tendency to make some sort of a response to each in turn. This tendency was of course diminished with adaptation to the experiment, but there always seemed to be some sort of response, either central in taking a new "set" for the final stimulus of the succession, or peripheral in pressing a little harder on the reaction-key, or both. When the intervals were reduced to 90 and 45^σ the feeling of this disturbance was scarcely present and the reaction-movements appeared more regular and much easier to the one who was reacting.

It was said in an earlier division of this work that the delayed response to light when it was preceded by other stimuli might be explained by supposing that the usual motor discharge had been partially drained off by the other stimuli.[27] It seems that each of the successive stimuli produced some sort of a response that was especially noticeable, when the intervals were long, as a slight movement forward of the body or an extra pressure of the reacting finger, and endured for a time like a positive after-image[28] and interfered with the response to light when it appeared as the cause of the overt reaction-movement.

[27] McDougall, "Physiological Psychology," 37, 1908.

[28] Sherrington, *op. cit.*, 34.

X.

General Summary

1. The reaction-times to light, electric shock and sound are in the order of their magnitudes: to light, longest; to electric shock, medium; and to sound, shortest.

2. The reaction-time to two simultaneous stimuli is less than to either of them presented alone.

3. The reaction-time to a group of three simultaneous stimuli is less than the reaction-time to any single component member, and less than the reaction-time to any paired presentation of the stimuli of the group.

4. When another stimulus is added to a single stimulus or to two simultaneous stimuli to which reaction has been made, there is a reduction in the original reaction-time. The amount of this reduction is dependent upon the reaction-time to the stimulus added, *i. e.*, if the independent reaction-time to the stimulus added is relatively the longest, the reduction is the least; if its independent reaction-time is the fastest, the reduction produced by it is the greatest.

5. The addition of a pair of stimuli to a single stimulus, to which reaction has been made, produces a reduction in the original reaction-time that is dependent upon the reaction-time to the pair added.

6. There seems to be no evidence that a reagent is able to select from a pair or group of simultaneous stimuli one of its members for reaction.

7. When stimuli of medium intensities are presented with others of low intensities, there is a decrease in the reaction-time.

8. The reaction-time to a light stimulus preceded by a sound and electric shock stimulus, at regular time intervals, is longer than the reaction-time to the light alone. A reduction of the time intervals between the successive stimuli produces a reduction in the reaction-time.